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THE
DUBLIN JOURNAL

OF
MEDICAL SCIENCE;

EXHIBITING
A COMPREHENSIVE VIEW
OF THE
LATEST DISCOVERIES

IN
MEDICINE, SURGERY, AND THE COLLATERAL
SCIENCES.

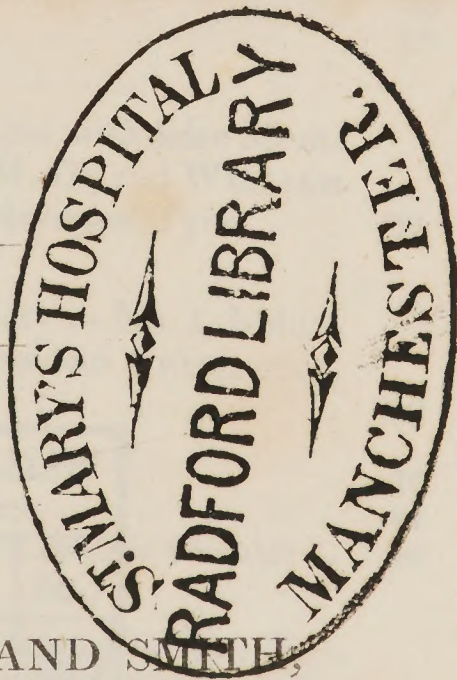
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We beg to acknowledge the receipt of several Numbers of the Philadelphia Medical Examiner, and of the Select Medical Library, and Eclectic Journal of Medicine.

We have received Dr. Genest's letter, with his edition of Professor Chomel's *Lecons de Clinique Medicale*. To Dr. Genest's proposal of exchanging the *Gazette Medicale* for the Dublin Journal, we gladly accede.

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The number and importance of the Original Articles in this Number, has obliged us to curtail our Bibliographic Notices and Scientific Intelligence.

Dr. Law's paper on the Use of Mercury in small Doses, will appear in our next Number.

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We have received two reclamations from Authors of Works noticed in our last Number, but as they do not in any way change our expressed opinions, we decline inserting them. They are to be had at our Publishers.

Dr. Willis's Work will be reviewed in the next Number, with several others which have come to hand.

Dr. Collins's Reply to Dr. Hamilton's last Paper, will appear in our next Number, and will terminate the important controversy between these eminent physicians.

THE
DUBLIN JOURNAL

OF
MEDICAL SCIENCE,

1 SEPTEMBER, 1838.

PART I.

ORIGINAL COMMUNICATIONS.



ART. I.—*On the Efficacy of Pressure in certain Cases of Venereal Phagedænic Ulceration.* By HUGH CARMICHAEL, A. M., Member of the Royal College of Surgeons in Ireland.

THE following cases of venereal phagedænic ulceration, which have yielded to a plan of treatment I believe to be novel in this disease, will not, I trust, be unacceptable to the profession. The destructive character of this description of ulceration, and the ravages it so frequently commits, are on many occasions such a source of embarrassment in practice, that I consider it sufficient to insure an interest for whatever may appear capable of controlling it in any instance.

Phagedænic ulceration has, of late years, been investigated with much care and attention by different writers on the venereal disease, and the pathological light, which, to a certain extent, has been thrown upon it, has led to an acquaintance with its real nature, by which our treatment of it is materially improved. Among other things, the indiscriminate use of mer-

cury, at one time so general in all its stages, is now admitted to be erroneous, and the abstaining, under certain circumstances, from its exhibition, and employment of remedies more suited to its real character, has been productive of consequences in the treatment comparatively fortunate. But although the almost uniform, and more or less rapid destruction of parts from phagedænic ulceration, is now very much modified under improved treatment, nevertheless, it is much to be regretted, that cases are by no means unfrequent, which defy altogether our skill, and where in despite of the most judicious management we are aware of, the termination of the complaint is unfavourable.

The truth is, as yet we are unacquainted with any remedy upon which we can satisfactorily depend for the controlling of this description of diseased action, and although by proper management, assisted by an unimpaired constitution, we are often enabled to arrest its progress in sufficient time to save the part, or some of the part engaged, nevertheless, ere this is accomplished, extensive destruction frequently takes place, while in many instances its entire removal is effected before the ravages of the disease are subdued.

That mercury is injurious during its existence in certain of its active stages, is, I believe, now pretty generally admitted, and acted upon; but, on the other hand, the withholding it, although it gives a better chance of success, by no means universally insures it : in order to this, something more is required ; is not enough that we should know and avoid what is injurious, we should likewise be acquainted with what is beneficial, and that we are but indifferently informed upon this, and that consequently our knowledge of the treatment must be considered still but imperfect, will scarcely be disputed, when such instances of its destructive termination are so constantly occurring.

Great irritability being one of the most prominent features of the disease, I was induced to imagine, that pressure, an agency used with so much benefit in ill conditioned, unmanageable ulcers generally, where morbid sensibility is a very leading

character, might probably, in these, be likewise adopted with some advantage; it was accordingly tried in a case that occurred to me of the most hopeless description, (Case I.) where all the varieties of treatment now generally employed were resorted to without effect, the disease progressing, and rapidly destroying the part, and the success which attended it was so decided, that I have since used it in several others, and with such benefit, as to establish it, in my mind, as a method highly deserving of attention in these cases.

The great obstacle I have experienced in its use, is the occasional difficulty of its application, so as to bring the diseased part decidedly under its influence; by a little dexterity, however, we may, in most instances, succeed in doing so, the operation sometimes requiring more management than at others. The mode of effecting it, of course, will vary according to the part the ulcer to be compressed is situated on; when on the glands or body of the penis, strips of adhesive plaster are the means I have adopted, looped, by passing one of the tails through a slit in the other; the penis is then to be introduced into the loop, and the ulcer being brought into its bearing, it may be tightened at pleasure; the tails are to be then firmly wound round the penis and secured. When on other parts, as the forehead, or places similarly circumstanced, (Case IV.) pressure may be more easily and decidedly commanded, while on others it may, perhaps, be more difficult to effect it; yet by management, I think, with very few exceptions, it can be accomplished in all.

On some occasions, where the required pressure should be more decided, I have employed slips of sheet lead, placed over any appropriate dressing, and included in the loop. This substance, from its pliant nature, admits of being easily moulded into any form, and can readily be shaped so as to produce effectual compression upon the ulcer. Indeed I think that the beneficial effects to be derived from pressure, particularly in ulcers, is not so much from the degree of tightness with which it is used, as in the application of a solid unyielding substance to

the surface, probably thereby inducing the absorption and removal of such diseased surface, and for the reasons just stated, sheet lead I have found to answer best for that purpose.

With respect to the time required for its continuance before its full effects were obtained, it was various; sometimes a few days changing the entire character of the ulcer from an ill-conditioned, spreading sore, to one of a florid, healthy aspect, with contracting boundaries; while on other occasions, it required a longer continuance; but in all, the amendment was so evident after the second or third day, as decidedly to manifest its salutary influence, and give assurance of a favourable result. In some instances I have been enabled, by means of it alone, to perfect the healing of the ulcer, unaided by any other measures, while in others, (and these the greater number,) its phagedænic nature was only removed, a morbid diathesis still remaining, which appeared incapable of being overcome entirely without more active remedies. In these latter, I found mercury to serve all the purposes required, the phagedænic character being first subdued, the regenerated sore rapidly disappearing under its influence, when the system became engaged by it; indeed it would seem as if its use were necessary in them to complete the cure.

In the cases in which I employed it, no other means were adopted, and in all it was successful, I mean so far as subduing the phagedænic disposition; some, however, no doubt may occur, where it could not be used alone or in the first instance; for example, if great inflammation were present, leeching, with a view to the subduing it, according to the suggestions of Mr. Richard Carmichael, who has advised local bleeding in these cases, would, I think, first be necessary, an instance of which will be found in Case II., and other circumstances might also be attendant upon it, requiring appropriate remedies before submitting it to pressure; these, however, could only be regarded in the light of preparatory steps, previous to the employment of this latter measure, and which must be considered, that whereby effectual benefit is to

be obtained ; perhaps there may be cases where it would be productive of no advantage, or altogether inadmissible. My present object being simply to state the great benefit I have derived from this agent, in such cases of the disease as I had an opportunity of trying it in, it would be foreign to my present purpose to enter into any discussion as to the probable nature of phagedæna, as met with in venereal affections. I may be permitted, however, to remark, that from the change I have observed to be produced by it in these ulcers, in some being sufficient for their perfect removal, while in others, though not so successful, yet producing such an alteration as to render mercury, not only admissible, but decidedly useful, and that in cases where, before its employment, that mineral was altogether contraindicated, an argument might be deduced, which would appear favourable to the opinion of those who contend, that phagedæna is rather a quality superadded to venereal ulceration, from some accidental cause, than a distinct description of disease in itself.

For example, in these cases where, though severe, the pressure was sufficient to complete the cure, it would seem as if the original sore was simply an excoriation which took on the phagedænic disposition, and that this disposition, being removed by employment of the means here spoken of, it was restored to its original character, and yielded to simple measures. Whereas, in those which, after they had ceased to possess the phagedænic character, in consequence of being submitted to this treatment, still, however, continued obstinate in other respects, resisting every other thing employed, and ultimately yielding to mercury only ; in these, I say, it would seem as if the ulcer, originally chancreous in its nature, became, in its progress, like the former, imbued with the same destructive and spreading disposition, and by the intervention of pressure this quality being removed, it resumed again its chancreous character, and then quickly yielded to the mercurial treatment.

These speculations, however, I admit, may be considered premature, where the opportunities I had of observing upon it

were perhaps few, and therefore I put them forward in the light I have, and rather with a view to induce investigation, than otherwise; but as far as I had opportunities, appearances were such as would be favourable to them, in the less obstinate the surface of the ulcer under its influence becoming soft, florid, and easily managed, while in those where the administration of mercury was required for their ultimate removal, after what may be termed a more healthy action had been produced, they had all the appearances of a healing chancre. I have again, however, to remark, that the number of cases are perhaps too few, to admit of general conclusions, nevertheless, should a more extensive experience produce similar results, it must, I think, tend, in some degree, to the elucidation of this disputed point; could it in any way reconcile the discrepancy at present in existence, in the opinion of practitioners equally entitled to attention, as to the propriety or impropriety of treating these cases by mercury, on the supposition that they have come to these different conclusions, from observing the effects of the mercurial treatment in the different stages of the disease here contended for; so long as the phagedænic character is dominant, so long perhaps ought mercury to be abstained from, and its use be injurious, but when this to a certain extent is subdued, and which probably time and palliatives might in many instances effect, then may it with safety and advantage be resorted to. Under such a view of the matter, the treatment of this disease might perhaps be divided into two stages, first, the subduing the phagedenic character, and then the cure of the ulcer. The former of those should, therefore, be the first object of the practitioner, as I have said in the outset, it is the great object to be held in view, the rest of the treatment being comparatively easy when this is effected. Whatever then can produce this change in a phagedænic ulcer, can alone be considered as a remedy in the disease, and as an exemplification of the powers of pressure in this respect, I shall now proceed to lay before the reader the following cases.

CASE I.—George Ryan, a bricklayer, about 27 years of age, consulted me for a phagedænic ulcer which occupied the side and under part of the penis; half of the glans and the entire of the prepuce were already destroyed by it, together with a portion of the adjoining part of the body; he states that it commenced in the form of a small sore on the outside of the prepuce, which became very irritable and spread, until at length, after a protrusion of the glans through an opening in that appendage, the disease went on to the extent it exists at present; he has been for some time in one of the large hospitals in this city, using cicuta, sarsaparilla, and antimony, together with mercurial fumigations, and the occasional employment of strong escharotic washes of various kinds to destroy the surface, but without effect; and on being informed by the attendant surgeon that there was no chance of saving the part, he left it.

At present the ulcer is so deep and extensive, that the first joint of the thumb might be buried in it; extreme irritability; the removing of the dressings produced the greatest pain, and occupied nearly half an hour; an unhealthy purplish red appearance surrounds the ulcer to some extent, the evident precursor of progressing phagedæna; worn out aspect from sleepless nights; the surface of the sore is covered with a glassy slough of a dirty yellowish colour, dark towards its edges.

The usual remedies having been tried without any effect, and impressed with the opinion I have stated in the foregoing remarks respecting pressure in these cases, I determined on trying it in this instance.

A long strip of adhesive plaster was accordingly looped in the manner already mentioned, and the ulcerated part being introduced into the loop, it was tightened so as to produce considerable pressure upon the sore, and the tails being wound round the penis, a firm compression was formed on it; during this operation the patient experienced most severe pain in consequence of the extreme morbid sensibility of the part; in about half an hour, however, it began to subside.

The day following, he stated, that his general feelings were quite changed, and that he had not had such comfortable sensations for several weeks before ; pain so much abated as to permit him to pass comparatively a most tranquil night ; he was able to walk about his room with a degree of confidence long unknown to him ; dressings not disturbed.

Third day.—Character of the ulcer on removing the dressings much changed ; appearances not what can be called decidedly healthy, but the destructive phagedænic nature so checked as sufficiently to mark the salutary influence of the treatment ; granulations partially appearing, and irritability so much abated as to permit the dressings to be quickly removed ; dressed as before ; pressure more firm.

Sixth day.—Decidedly healthy aspect ; surface in most parts red ; edges of ulcer levelling with surrounding parts ; surface touched with sulphate of copper, and dressed as before ; after this the ulcer under this treatment became daily more tractable, and its syphilitic character, if at any time it was so embued, being now as it would seem gone, the ultimate cure was slowly but easily accomplished, preserving the portion of the glans unaffected when I first saw it, and which during the cure was considerably regenerated.

Observations.—This case having completely yielded to the pressure without any other remedy, I should take it to be one which was free from syphilitic taint at any time.

CASE II.—A young gentleman, about 20 years of age, applied to me on account of a large indolent pustule on the outside of the prepuce ; he informed me it made its appearance three weeks before in the shape of a pimple, and slowly increased to its present extent : as it had not the characteristic of genuine syphilis, I directed stupes and poultices, to observe perfect rest, stating my apprehension that it might become a troublesome ulcer.

About a week after he returned, and it then had increased considerably ; it was now about the size of a large bean, some-

what of that shape, of a yellowish brown colour, the scab more moist than when last seen.

Unfortunately his avocations required him at this time to leave Dublin for the country, where he was occupied nearly three weeks in the active employment of an assizes town. On his return I found the phagedænic ulceration quite established, the sore increased considerably ; in fact one side of the prepuce had fallen a sacrifice to it, and that side of the glans was deeply engaged, the ulcer deep and spreading, and its surface covered by a dirty, yellowish tenaceous slough.

The extreme irritability, the inflammatory appearances of the parts, and the general state of his system, from the busy mode of life he had been engaged in for the three preceding weeks, made me hesitate about putting in practice the plan I adopted in the last cases, although my confidence in it was so decided from the advantages I experienced from it in others ; I therefore enjoined the most perfect rest, the use of cooling aperients, with appropriate sedatives, and had two leeches applied daily for three days to the part ; by this, inflammation was somewhat got under, but the irritable, destructive nature was altogether unabated.

The body of the penis was therefore now enveloped by a long strip of adhesive plaster, in the same manner as in the former case, and with the same pain to the patient ; three days after, no amendment, but no advance in the disease ; the ulcer, however, being deep and irregular on the surface, a small piece of sheet lead was shaped so as to adapt itself to its irregularities, and after wrapping it in adhesive plaster, was applied on the sore, and firmly retained on it by a slip of the same plaster. This dressing was permitted to remain undisturbed for two days, when a slight improvement appeared on the surface, and by steadily following up the treatment, at the expiration of ten or twelve days, the unhealthy character was altogether gone ; after this the healing of the ulcer was slow, but proceeded steadily until it entirely disappeared. There was no mercury

used in this case, nor did its employment appear indicated in any stage of the treatment.

I had an opportunity of observing the subject of this case after the cure was accomplished by the above means : in about five months after, he was attacked with the most severe constitutional symptoms ; they were, however, confined to distressing periostitis, venereal cachexy, with the peculiar yellowish appearance of the scalp, and shining blackness of hair, so characteristic of severe constitutional venereal derangement ; for more than two years he was harassed in this manner, symptoms threatening consumption having set in ; by change of air, however, sarsaparilla, and the use of the hydriodate of potash, combined with Dover's powder, the symptoms have entirely disappeared, and he is now in the enjoyment of perfect health. I never saw a case of phagedænic ulceration so apparently hopeless as this, the destruction of the entire of the penis having appeared inevitable, and indeed for some days I almost despaired of the result. I have no doubt that had I relied on the remedies at present employed in these cases without resorting to pressure, the entire of the penis would have fallen a sacrifice.

CASE III.—I was requested by the late Surgeon O'Hara, to whom I had communicated the beneficial influence I had found pressure to exercise over these ulcers, to see a gentleman under his care affected with this formidable disease. The patient, a thin person, much reduced in health from the effect of the complaint, had an extensive phagedænic ulcer on the under part of the penis, which had already removed the frænum and extremity of the glans, and wound round to the dorsum, in its track engaging and deeply destroying the intermediate parts. The usual excessive irritability was here present, rendering the least movement most distressing ; there was, moreover, this peculiarity in the case, that it was accompanied by a paraphymosis. It appears the original sore was situated on the inside of the prepuce, and on one occasion, when the patient retracted it for the

purpose of washing the part, he could not replace it, and before he sought advice, the suffusion and induration was so great, that it could not not be reduced.

Under the directions of Mr. O'Hara, all the usual remedies were resorted to, but without any benefit, the disease gaining ground on the parts, so that their utter disappearance before it seemed likely to take place at no very distant period.

The disease appearing to be aggravated by the constriction arising from the paraphymosis, my first endeavour was its reduction, but under even more than the ordinary force I found this impossible, and therefore at once proceeded to the employment of pressure, with all the disadvantages arising from the state of constriction. The sore was accordingly covered with a piece of sheet lead, and this secured tightly, by a strip of adhesive plaster, in the way mentioned. This plan was pursued for one week, but without any benefit; however the disease made no progress, and at the end of that time, seeing no apparent approach to amendment, I concluded that the beneficial influence which this mode of treatment might have produced, was probably kept in abeyance, by the constriction induced by the paraphymosis, and therefore decided on its reduction if it could be effected. For this purpose, the necessary steps being taken, I attempted the replacement of the prepuce, using a degree of force in the attempt amounting nearly to violence, attended of course with much suffering to the patient, but without success; the further attempt was therefore abandoned, and a light dressing placed on the sore: it will be recollected, that the state of the ulcer was nearly as unfavourable, as when I saw it the first time. On our visit the following day, however, our expectations were most agreeably disappointed, by finding the character of the ulcer altogether changed, a manifest healthy action having set in, which in less than a week was so decided, that the sore was healing: it became necessary, however, in order to complete the cure, to put the patient under the influence of mercury, the curative process, after it had gone on to some ex-

tent, becoming stationary, and the sore having the appearance of a healing chancre, which rapidly disappeared on the system becoming affected. This case I should conceive to have been connected with a syphilitic taint.

Observations.—The beneficial influence of the mode of treatment I here recommend, is, I think, peculiarly illustrated in the case just detailed: the apparent failure of the pressure was evidently the result of the unnatural state of the parts, in consequence of the presence of paraphymosis, but immediately on being submitted to the force which the second attempt at reducing it necessarily required, and which must be looked on as operating upon the ulcer in the same way as pressure, only in a more decided manner, its salutary influence became at once evident, and the total disappearance of the morbid phagedænic character took place, with more expedition than in any other case I have seen it tried in. It is surprising how great the regeneration of parts invaded by this disease is, particularly of the glans, once that healthy action is restored; in this case, although a large portion of it was destroyed by it, the traces of the disease were scarcely discernible after a short time.

CASE IV.—As the following case differs from those related, being this disease in a constitutional form, and likewise invading and destroying the surrounding parts more quickly than in them, I shall state it more fully, and with reference to dates; it appears to me, moreover, to offer more decided proofs of the efficacy of the treatment.

October 9th, 1837. Patrick Maguire contracted a venereal complaint fourteen months back on the edge of the prepuce, for which he was admitted into an hospital at Liverpool, where he used mercury so as to sore his mouth, and the ulcer having healed, its further use was discontinued. Four months after, he was attacked with pains in the joints and bones, for which he also used mercury, but without much benefit, and his throat becoming sore, he applied to me. His general appearance being very much reduced, the mercury was discontinued, and the hy-

driodate of potass, with Dover's powder, substituted, touching his throat freely with creosote, according to the plan recommended by Sir Francis Smyth, by which his complaints disappeared. In four months after this, he again applied to me, his appearance greatly reduced, a large phagedænic ulcer situated over the right supercilium that spread downwards, so as to engage the eyelid, which it had completely eaten through under the superciliary ridge, so that on depressing the lid, the upper part of the globe was exposed: there was also another ulcer of the same description upon the lower lip. These ulcers were covered with a softish, ill-conditioned, yellowish scab, which, upon slight pressure, yielded from under it, a thickish purulent matter, and on removal, displayed a deep irregular honeycombed ulcer, extremely sensitive to the touch, with high jagged edges. He states they first appeared about six or seven weeks past, in the shape of an irritable pustule, which after some time began to spread. On inspecting the position of the primary sore, a hard, knotty tumour, about the size of a large pea, is discovered, the remnant of an ill cured chancre.

The ulcer on the supercilium alone was put under treatment, leaving that on the lip unattended to, and a piece of lint, soaked in oil, being laid on it, covered with a portion of sheet lead, the entire was secured by a bandage rolled round the head, with a moderate but firm pressure, causing much pain to the patient.

11th. There is an evident amendment, the scab has disappeared; the superior part of the ulcer, particularly, that had been steadily under the influence of the pressure, is improved, and thickly set with granulations; its high jagged edges levelled with the surrounding parts, the portion of it, however, occupying the lid, and which in consequence of its peculiar position, it is difficult to command, still retains its original character. Dressed as before; the lid particularly attended to, to bring it under the influence of the pressure if possible.

13th. Still further amendment; all parts look healthy, and partially granulating; but although phagedæna has very much

disappeared from the ulcer, except at two small points at the lower edge, there is a morbid appearance on it which calls for more than simple means. Pressure continued.

Pilul. Hydrarg. gr. v. ter in die.

20th. Up to this period the ulcer has been improving, so far as an evident syphilitic ulcer, by which it is now characterized, could improve, without the system being mercurialized. The mouth is sore to day, and the amendment very considerable. These observations apply only to the sore on the forehead, which alone was treated by pressure ; that on the lip, and which was not attended to, underwent no change up to this, but to-day appears more irritable ; although, therefore, the present decided improvement in the former must be considered as the effect of the mercury, the state of that on the lip, however, shews, that it alone would be insufficient, but that the local treatment by which it was preceded, was chiefly instrumental in producing it, by inducing a condition of parts which fitted them for the salutary operation of that medicine. Pressure discontinued.

30th. The ulcer on the forehead is now nearly well, and is touched with the sulph. cupri ; that on the lip, which grew worse on the system becoming affected with mercury, improved afterwards when the mercurial action was declining, and is now somewhat better.

November 4th. The sore on the forehead is quite well ; the lip is worse than at the last note ; the mouth is well from the mercury.

28th. He came to me to-day ; the forehead perfectly well ; the lip again bad, in an angry, irritable state ; the hardness at the seat of the primary sore is quite removed ; the part soft and pliant.

December 10th. The ulcer on the lip is worse than at any former period, the right half engaged in it ; covered with the yellowish soft crust already spoken of ; the lip swollen ; great

irritability. He states he had no sleep for the last three nights, in consequence of the pain he experienced from it; pressure applied by adhesive plaster and tight bandage.

12th. The scab entirely gone; no apparent improvement, but irritability much reduced. Dressed with lead compression.

14th. Still no decided amendment, though crops of red granulation appear scattered over the ulcer; the yellowish, tenacious, sloughy matter still covers it in some places; the compression with lead appears not so serviceable; adhesive plaster and tight bandaging only employed, and surface touched with sulph. cupri.

16th. The ulcer now decidedly improved, the swollen state of the lip nearly entirely gone; dressed as before.

21st. The phagedænic character appears now subdued; the ulcer, however, not disposed to entirely heal, in consequence of the chancreous nature already spoken of; its surface, however, florid in all parts.

Pil. Hydr. gr. v. ter in die.

After this the sore was dressed simply, without pressure in any form; in about a week his mouth became sore, and he called on me on the 8th of January perfectly well.

Were it admissible to theorize from a single case, that just detailed affords the most ample proof of the influence pressure possesses over the phagedænic ulcer; the sore on the lip, unattended to while that on the forehead was under treatment, continued unaltered, at least made no progress towards a cure, while the other was perfectly removed.

When the system became affected with the mercury exhibited for the latter, the former grew instantly more irritable, and continued so during the continuance of the mercurial influence in its active state, while that on the forehead, the phagedænic nature of which had been previously removed by pressure, grew rapidly well under it, nor did the ulcer on the lip manifest any disposition to improve in any particular, or

change from its state of newly created irritation, till the mercury was subsiding in the system.

The suspension which then ensued with respect to its condition was, however, but of short duration ; on the system becoming free from the mercurial action, the phagedænic disposition was again renewed in it, and in a more aggravated form, and threatened the total destruction of the lip. On resorting, however, to pressure, this morbid nature in it was destroyed, and mercury, which before had been so ineffectual with respect to it, now exhibited the most salutary influence, decided healthy action at once set in when the system was affected by it, and the ulcer was perfectly cured and continued so.

I could further elucidate the salutary influence of pressure in these ulcers by the recital of a few more cases ; those, however, which I have stated are, I think, sufficient for my present purpose, without intruding unnecessarily on the pages of a public journal. We are not, I must observe, to expect a very quick and decided change in every instance from it, the amendment being frequently slow but ultimately certain. We must persevere, however, in its use, when it ultimately destroys the phagedænic disposition, and the sore quickly yields to mercury ; indeed when we consider the probably complicated nature of the ulcer we have to treat, (syphilis imbued with phagedæna,) this obstinacy of character, as referrible to this treatment only, may naturally be expected. The pressure, for example, I conceive to be alone capable of controlling the latter, but in no way of influencing the former, which may rather come into action as the other is subdued, and so frustrate the healthy disposition that might otherwise set in. The occasional difficulty of its application, so as to command the entire of the ulcerated surface, and thereby of obtaining its full advantage, will likewise require to be taken into account in estimating its value, as also to what extent it may be available, whether only in certain particular instances or more universally, a point to be decided by future trials, but which the destructive nature of

these ulcers will induce particularly when other remedies have failed; but perhaps the principal matter to be derived from it will be the suggestion it may offer towards the better understanding their true nature, now so much disputed by the best authorities, but to enter upon which, or to attempt to explain from the effect this mode of treatment may have upon them, must be premature, until more extensive information be collected upon the subject.

ART. II.—PROFESSOR DIEFFENBACH *on Excision of the Bones of the Face.* (From the *Zeitschrift für die Gesamnte Medicin*, No. 2, February, 1838.)

THE Professor modestly asserts in the commencement of this paper, that it is only a few years since he had applied his attention to the extirpation of diseased portions of the under jaw, and afterwards to that of other bones of the face, and that therefore his experience is but slight, whilst the great improvements which have been made in this branch of surgery he attributes to Jaeger. There is, however, so much of originality in the methods pursued by Dieffenbach in the performance even of common operations, that we always hail with pleasure any thing proceeding from his pen, which is generally as terse in description as his hand is quick and sure in operating. The first case in which professor D. undertook to remove a portion of the alveolar process for osteosarcoma, occurred in a man forty-eight years of age, around the incisor tooth of the left side of whose jaw, a fungous enlargement had occurred. The tooth was easily moved in the fungous mass, and when stirred, blood exuded. “I sawed out the diseased portion of the bone in the shape of a wedge, with a small saw, and then burned the surfaces of the wounded bones with a red hot iron. After some time, a few splinters of bone exfoliated; healthy granulations appeared, and within six weeks, the aperture very much diminished, was

covered with a small cicatrix, and the man got well, without any return of the complaint."

CASE II.—A woman, æt. 38, who also had suffered from an osteosarcoma of the alveolar processes, which, in the space of a year, had increased to the size of a walnut. I excised the edge of the jaw with three teeth, one molar, one canine, and an incisor. The actual cautery was employed in this case also, partly to stop the bleeding, and partly to cause exfoliation on the cut surfaces of the bone. A perfect cure was effected in two months, and there has been no recurrence of the disease.

CASE III.—I sawed out a portion of bone, of the size of a walnut, in a state of osteosarcoma, together with two incisor and the left canine tooth of a female, æt. 46. In this case, also, after the actual cautery, exfoliation of some bony fragments occurred, after which the wound healed with a smooth surface.

CASE IV.—A lady, æt. 40, had a hard sarcoma, of a dark bluish aspect, surrounding the roots of two incisor teeth, which had formed gradually. The diseased part was sawed out, and the incision cauterized. Recovery ensued without recurrence of the disease.

CASE V.—An osteosarcoma, the size of a large walnut, had developed itself in a delicate young woman of twenty-four years of age. It occupied the space of three teeth. I cut out the bone by an angular incision, opening into the antrum high-morianum, and including a portion of the hard palate, the bones of which were softened: the wound was cauterized. A cure was effected within three weeks. This case was novel, and as yet there has been no recurrence of disease.

CASE VI.—A softening of the alveolar process of the left upper jaw, occurred in a delicate fair girl of twenty-four, and had existed about a year. Numerous internal and external remedies had been applied in vain; a molar tooth, which had become loose, had been drawn, the most prominent part of the fungous surface cut away, and the wound cauterized, without success. The disease continually progressed, and threatened to

pass to the zygoma. The teeth shewed only the upper surface of their crown out of the fungous mass, which was particularly developed on the side next the throat. If the teeth were laid hold of and stirred, blood oozed from beneath them. Without cutting any external parts of the face, I excised the whole left edge of the jaw, as far as the incisors, with a small saw, cutting through the sound bone, and stopped the violent bleeding with the cautery. Some fragments of bone exfoliated; healthy granulations shewed themselves every where over the surface of the incision, and within five weeks the girl was perfectly cured, and was not at all disfigured externally. After the operation, she became healthy and blooming, and now that a year has passed over, still continues so.

CASE VII.—Madame B., a lady past fifty, suffered for some years from a fungous softening of the greater part of the alveolar process of the upper jaw, but this had principally engaged the anterior part of its edge. She had been treated by many physicians, both with internal and external remedies, and the loose teeth had been extracted one after another. At present the whole border of the upper jaw is converted into a lardaceous-looking tumour, which the upper lip hardly covers. The patient is weak and dispirited.

As soon as I had separated the upper lip from the tumour, and had turned it upwards, I sawed off the alveoli as far as I found them diseased, and cauterized the wound. After some months, this patient appeared to be cured. She recovered, by degrees, from her exhaustion, and I considered her already perfectly cured, when the cicatrix appeared again, and became covered with new fungous granulations. Astringent washes, pencilling with ext. saturni, and the application of nitrate of silver limited the extension of the swelling, yet the bones still higher up had become soft, and the patient removed herself from mine and Dr. Halthof's care.

CASE VIII.—A young lady, ætat. 23, had for some years a thickening of the left upper jaw, between the ala of the nose

and the malar bone, which was caused by an abscess of the bone. I separated the cheek, commencing at the mouth, from the bones, and removed the anterior surface of the back of the abscess, and the edge of the bone. That part of the bone which was deeply seated was cauterized. A cure was effected.

CASE IX.—An enlargement of the bony palate had developed itself for many years in a man of thirty years old. This swelling was convex, and like half a hen egg. An incision was carried round it with a knife, and it was removed. The bones of the palate, which were much forced out of position, had a small aperture in the centre. The actual cautery was applied deeply. The aperture became filled with granulations, and the palate, after it had healed, regained a natural aspect. Pronunciation, which before had been very indistinct, after the operation became quite natural.

CASE X.—The under jaw of a man, æt. 60, had for many years been enlarged to an incredible size in many parts. This was caused by hydatids forming between the inner and outer lamellæ of the inferior maxilla. Very frequently these inflamed, and passed into a state of suppuration, and were then treated as simple abscesses by incision, after which the formation of matter ceased, and the man recovered. The great age of the patient forbid a radical extirpation.

CASE XI.—A man of 30, for a number of years had been quite an object of curiosity, on account of an enormous tumefaction of the left cheek. This tumour projected from the face as large as two fists. The upper part was hard, the lower elastic, and the disease had taken ten or twelve years to arrive at its present size. I thought that I recognized in this case, a forcing asunder of the bones of the face by an hydatid, and on that account, I removed the portion of the tumour which projected into the mouth, and through this opening I extirpated also the greater part of the cyst, and the sharp edges of the bones. Abundant suppuration succeeded, yet as cicatrization proceeded,

the cheek, which had fallen in, recovered its natural proportions, and the patient was cured without the least deformity.

CASE XII.—An osteosarcoma of the left zygoma developed itself with much pain, in a man of nearly sixty years of age. at last it broke through the skin, and the fungous bone lay bare ; Neither internal remedies, nor powerful applications and caustics, produced the least effect on it. I circumscribed the diseased part with the knife, and sawed out the greater part of the zygoma. By cutting loose the adjoining sound portion of the skin of the cheek, I was enabled to cover a great portion of the wound. Within three months the part was healed with slight deformity ; I then lost sight of him, but afterwards heard, by chance, that he had died a year after the operation of dropsy.

CASE XIII.—In August, 1832, I was called to visit a foreign Jewish merchant with a long beard. He was sixty-three years of age. I found the second upper incisor surrounded with a soft osseous tumour, and I sawed the tooth with the alveola out. This man departed before he was perfectly healed. The incision, however, quickly closed. Three months later, a new osseous fungus arose out of the chasm ; an intelligent surgeon extirpated it, whereupon the place cicatrized. In May, 1833, another tumour was removed, and the place cauterized ; after this had completely healed, the disease recurred in Autumn, so that a new operation and cauterization became necessary. Fourteen days after which, a new fungus had to be burned away.

Notwithstanding those repeated extirpations and applications of the cautery, the softening and enlargement of the edge of the jaw, extended over one-half of the right jaw bone, to the nasal process of the upper jaw. In order to reach the diseased bones the soft parts over them had first to be divided. I made an incision through the cheek, commencing above at the nasal side of the lower eyelid, and carrying the knife downwards through the upper lip. The lip and cheek were then freed

from their connexions and turned aside, and the cartilage of the nose, which had been separated, also drawn to the opposite side. The diseased bones were now sawed out to a considerable depth, and the muscles, which on this side had undergone a fatty degeneration, were removed with the scissors. I applied the red hot iron firmly on the edges of the bones, and finished the operation by the application of a great number of interrupted sutures, with which I brought the separated parts of the face and upper lip together. After a few days I was able to draw out the pins, as the wound had united.

The cure proceeded also quickly in the aperture in the mouth, and a great part of the loss was remedied by flourishing granulations, to which a firm cicatrix followed. In three months the patient appeared to be cured, when the upper jaw, close to the nose, and a part of the septum, again softened and enlarged. I extirpated the diseased part again, and carbonized the edges of the bone with the iron. After they were thrown off the man got well. The defect in the bones was supplied by a well constructed mass of walruss tusk, furnished with a palate plate and a row of teeth, by means of which his speech was rendered perfectly natural. Externally there was no deformity in the face, and the only trace of the operation was a linear cicatrix.

This man then returned home, and two years after he caused me to be informed that he was in perfect health. Half a year later he informed me that the old disease had returned. Soon after he came to Berlin, having determined to submit himself, with extraordinary firmness, to a new operation.

I found that the old bony cicatrix, as well as the bones adjoining, had become much enlarged and softened, as had also a part of the left upper jaw which bordered on them. I repeated my former operation, slit the face close to the nose, and having everted the cheek flap on one side, and the nose and lip on the other, I sawed out the diseased bones again, and again applied the actual cautery. The incision in the face was

united by sutures, and this time also there was quick and good union. In some months the mouth was also healed. Externally there was no more deformity than after the previous operation. He then returned home, and has continued well up to the present time.

CASE XIV.—Mr. G. of F., a strong young man, æt. 22, was so much deformed in face, that he could not go into society. The left side of his countenance projected in the size and form of a small loaf, and had dragged the right so much, that the nose was carried far to the left. The right eye was pushed out of its cavity, by a swelling the size of an hen egg, on the top of which it was fixed. The everted eyelids had exactly the appearance of the external organs of generation in the female widely drawn asunder. There was perfect ectropium of both lids. The inner surface of the eyelids of a bright scarlet colour, surrounded and tightly compressed this tumour of the orbit, which carried the eye on its summit. The skin of the cheek was much distended by the violent strain, and full of varicose veins. The upper and under tumours were separated by a deep furrow. In other respects this young man was in the best health; none of his senses had suffered, and even with the eye so much pushed from its situation, he was able to see.

It is easy to conceive, that in such a case many surgeons had been consulted, and much medicine employed, without the least repression of the disease.

The most eminent persons, both at home and abroad, had seen this patient, and he had been treated not with homeopathic doses, for under the care of some he had taken 200 bottles of the decoct. Zittmanni,* and had sought a cure by starvation and

* Decoctum Zittmanni:

R Rad. Sarsaparillæ ℥xii. coq. c.

Aq. Font. lib xxiv. per $\frac{1}{4}$ h. dein adde

Alumin. Saccharat. ℥iss.

Mercur. Dulcis. ℥ss.

Cinnabar. Antim. ℥i.

inunction. But as none of these plans of treatment produced any decrease in the disease, he had not employed any remedies for two years. The tumour continued steadily to increase without giving pain. Convinced of the inutility of all kinds of medical treatment, I undertook the operation, considering the tumour to be of a fibrous nature.

I carried my first incision from the external angle of the eye outwards over the top of the tumour, and as far as the edge of the lower jaw, then I dissected the flap from the tumour which lay beneath, and turned it back ; it contained the flap of the cheek and nose, as well as the under eyelid, with the deeply drawn fold of skin which separated the two tumours. Now I began to touch upon the deep seated relations of the tumour ; I pursued them under the zygoma ; from thence passing round the body of the right superior maxilla, which was much pushed back, I came to the basis of the skull, and here cut the highest attachment of the tumour across. I severed other roots from the posterior nares, into which ramifications of the tumour of considerable size extended, and when they had reached the bony palate, becoming enlarged, they pressed upon it, and gave

In nodulo ligat : sub finem coctionis admisce

Fol. Sennæ ℥iii.

Rad. Liquirit. ℥iss.

Semin. Anis. Vulg.

———— Fœniculi āā ℥ss.

The first xvi. lbs. of this decoction, when filtered, constitute the Decoctum Forte.

The Decoctum Tenue is thus prepared :

R̄ Resid. Decoct. Fort.

Rad. Sarsaparill. ℥vi. coq. c.

Aq. Font. lib. xxiv. sub finem coctionis adde

Pulv. Cortic. Citri.

———— Cinnamom.

———— Cardamom. āā ℥vi.

Rad. Liquirit. ℥vi.

The first xvi. lbs. of this decoction, when filtered, constitute the Decoctum Tenue.

it a convex appearance. Then I began the removal of the tumour in the orbit; it was connected by processes which passed through the bony plate of the wall of the orbit, with the inferior tumour. I found it very difficult to support the eye, however I succeeded in freeing the bulb from all its connexions, and then I dissected the optic nerve from the hard tumour which surrounded it, as far as the optic foramen. The tumour still remained fixed in the walls of the orbit, and many hard cords which proceeded from it, passed through the shattered bones. However I succeeded in removing the whole tumour. The bulb of the eye, together with the optic nerve, which was extended to a long cord, now lay between my fingers, as cleanly dissected as in a preparation. My friend Romberg, distinguished for his writings on nervous diseases, and I, now made some experiments on vision. We caused the other eye to be closed, and then changing the position of the eye we found that wherever we directed it, the patient saw and distinguished clearly. As the bulb was now too small for the much enlarged orbit, which contained no bolster of fat, I first laid the optic nerve together in a spiral shape, and then pushed the bulb into the back part of the orbit, until it gently pressed on the bony walls. Then I modelled a moderate sized eyelid out of the immensely distended and thinned eyelid, which was covered over with varicose veins, secured it with fine pin sutures, and at the conclusion of the operation united the great flaps of the face with a considerable number of divided sutures, for which purpose I made use of stronger or weaker insect pins, according to the difference of thickness in the edges of the flaps.

In order to prevent the eye from falling out, the eyelids were closed, and brought into gentle contact with the ball of the eye, by means of a large soft ball of charpie which was pressed into the orbit.

The patient was now submitted to a very severe antiphlogistic treatment, was given saline purgatives, bled, had leeches applied in large numbers on the face, and day and night cloths,

wet in iced water, were applied. By this treatment at no period was life endangered ; the wounds healed quickly. On the second, third, and fourth days, the sutures were removed by degrees, as the edges had united by the first intention. In a few weeks the deepest parts were healing. The bulb of the eye, together with the eyelids, by means of a newly formed cellular tissue, had advanced to their natural position, and some time later, there was nothing to be remarked in this young man's face, than that the ball of the eye was a little crooked with respect to the opening of the lids, and that the cheek and angle of the mouth had fallen a little, the necessary consequence of cutting across the facial nerve.

Two years after the operation, this summer, Mr. G. visited me. There was much less paralysis of the cheek, and the angle of the mouth could be moved tolerably well. The appearance of the eye is perfect, the ball of the eye even is somewhat moveable, inasmuch as it follows the motion of the eyelids.

By this case I learned, that whenever it should be necessary, in my future operations, to divide the entire side of the face, and necessarily the facial nerve, that I would altogether avoid that method, and not divide the cheek, but the face exactly in the middle, and that I would do so even if the operation were to be performed at the posterior part of the cheek. I determined in the very next case which presented to cut down through the mesial line of the face, and having slit the nose and lip, and thrown back the half mask, to finish the operation.

I had the three following opportunities of performing this operation.

CASE XV.—Madame H., æt. 54, often unwell ; remarked for many years a stoppage in the left nostril, through which at last she could not breathe at all. The nostril then became perfectly stopped, and a dark blue tumour could be seen far back in it. Internal and external remedies were of no service.

The lady now came to me. I found a fungus melanodes which distended the left nostril, and by which the external

parts were violently pushed forward, and on their surface were many melanotic spots.

When the patient had used the decoct. Zitmanni for some time, I extracted with the forceps as much of the fungus as I could reach, and removed a great part of the mucous membrane which had undergone a fatty degeneration. The inner spongy bones of the nose were carious. The whole cavity, which had been emptied, was touched with a cauterizing iron of the thickness of the little finger. The patient was treated slightly antiphlogistically, and took the decoct. Zitmanni again for a space of six weeks.

For many months after the operation all appeared to go on well, and the cavity to heal ; then the dark fungus grew again ; it attacked the inner surface of the nasal bones and the upper jaw, particularly at the nasal process. The entire region became tumid, and the fungus appeared about to burst out externally, there being nothing to prevent it but the skin.

Three months and a half after this operation I performed the second. I cut through the soft parts immediately under the forehead, and carried the incision directly along the bridge of the nose, until it came to the upper lip which I also divided : cutting down to the bone, the flap formed of nose, cheek, and lip was thrown back, and the carious bones of the side of the nose, together with a portion of the diseased upper jaw, was sawed out. I then removed with the forceps, scissors, knife, and saw, all the remaining portions of the fungus from the deep seated parts, and even from the frontal sinuses, where some of it was rooted, as well as all the carious bones. When I had cut off a melanotic portion from the skin of the nose, I united the remainder, commencing at the brow, and ending at the upper lip, with a great number of pin sutures. The patient was treated with cold applications.

I gave the preparation of the parts removed to my distinguished friend J. Müller, who also visited the patient three days after the operation, and found her almost well ; on the fourth

day the whole incision in the face was united in a single line. Up to the present time, a year after the operation, there has been no return of the complaint.

CASE XVI.—One day a lady thickly veiled came to me, and expressed a wish to speak to me in private. I acceded, and when she had thrown her veil back, I thought I saw an immense gourd, for my eyes fell on a great round body, on the side of which was a wizened distorted face, furnished with a cocked nose, the left ala of which was enormously distended, and along with the cheek, formed a covering for the tumour. The eyelids were also greatly distended, the angle drawn obliquely; the whole skin was full of varicose veins. This disease had been gradually increasing from the eighteenth to the forty-eighth year of her life, until it had acquired the present size.

I split the face down the middle, commencing between the eyebrows. When the incision had been carried through the nose and upper lip, I made another oblique incision running parallel to the eyelid, passing over the root of the nose; then I freed the soft parts, that is to say, the half of the nose, the under eyelid, the upper lip, and the cheek from the tumour, till I came into the vicinity of the ear, and I turned this immense flap back.

The size of this tumour, which projected unequally on every side, would not admit of extirpation from its deep-seated relations, till a prominence the size of a fist was first removed with an amputation saw. Now first was it possible to apply a small thin bladed saw on the nose, and to saw through it as far as the orbit; I then sawed through the greater part of the under edge of the orbit with the maxillary plate, then cut out the zygoma, and sawed through the upper jaw obliquely from above downwards, till the alveolar process alone remained. When the deeper hard connexions had been severed with a knife saw, or were freed with the scissors and knife, I was able, with a little force, to remove the whole mass, and then a very

large and wide cavity remained. If before the operation I could imagine that I saw the outside of a gourd, so now I did not find it difficult to imagine that I was looking into the inside of one which had been hollowed out: right and left, except where the bones had been sawed out, the walls were perfectly smooth. All the spaces of the internal nares of the opposite side of the nose were involved in this great cavity; the posterior wall of the pharynx formed its last boundary. I now removed some large portions of the tumour out of the frontal sinuses, which were much softened, and in doing so much purulent matter was given exit to.

After having restored the lady, who now fainted, I united the severed face with an immense number of pin sutures. The eyelids and the angle of the eye were also united with very fine suture pins.

At first, some cordial medicines were administered, the face was lightly covered; internally wine was given, until she had recovered from her weakness, and next day, saturated alkaline solutions, as her constitutional symptoms demanded antiphlogistic treatment. The thin and deeply sunken soft parts became more elevated, and somewhat turgescient, next day; and on the third day, union was so perfectly accomplished that the sutures were all removed. In one spot only, between the under eyelids and the nose, the united flap gave way, for about the size of a silver groschen (nearly the size of an English sixpence); however, at a later period, I had hopes of being able to remedy this unpleasant occurrence.

Without any occurrence worthy of remark having taken place, this patient was so far recovered in a few weeks, that she was enabled to leave her bed.

When she had recovered, there were two things in her face, now become straight, to improve: one, the closing of the opening, and the other, the elevation of the eyelid, which was somewhat drawn down by the internal cicatrization. I succeeded in the former, after some failures on account of the

extreme thinness of the edges of the skin, and the want of a sufficient support underneath, by pairing off the edges, and uniting them by sutures, and drawing the adjoining skin close, having first made a lateral incision : lastly, the falling in of the eyelid was not quite restored. This lady, a year since, was in the enjoyment of excellent health, and went into society, which for ten years previously she had been obliged to avoid. There was no trace of any return of the disease. The immense cavity had diminished very much, and the countenance was natural and moveable on both sides. The tumour removed was of a fibrous nature. The bones were partly absorbed, and in some places were fixed in the tumour, in the form of thin sound plates.

CASE XVII.—The Ober Amtman K., æt. 50, had a bony tumour of the left side of the face, which had continually increased for some years. He had suffered little pain, but was never free from a dull sensation of pressure. The nose was drawn to the right side, and its left ala was much more elevated than the right, which was stretched over the ball shaped tumour. The skin of the cheek was of a bluish red, and perforated by many fistulous openings : the left nasal bone, the edge of the orbit, and the zygoma, felt softened. When the mouth was opened, the alveoli, all round the upper jaw, together with the whole of the hard palate, was found to be converted into a mass like lard.

This patient had been under the care of skilful surgeons, and had taken many remedies famed for their power of curing diseases of the bone, and finally a course of the decoct. Zitmanni ; still the disease proceeded regularly in its development, and was just on the point of bursting through the skin of the cheek. He then determined to travel to Berlin. I commenced the operation of splitting the face, and carried the incision through the nose and upper lip into the mouth. An oblique incision above was carried to the angle of the eyelids, and the under eyelid, half the nose, with the cheek and upper lip, were separated from the mass of softened bone, until the masseter muscle

lay bare. I then separated the whole of the right side of the upper lip from the bone, and sawed through the upper jaw, cutting upwards, and commencing at the middle of the alveolar process, till the saw had cut into the cavity of the nose; I then directed the instrument obliquely through into the orbit, and removed the greater part of its lower border and its inferior plate. Then I sawed through the zygoma, and with the saw, which I sometimes changed for the scissors, and sometimes for the knife, I worked into the deep parts, and excised, one after another, all the deep-seated parts of the upper jaw, the whole of the tumid hard palate and alveolar process, as far as the velum palati. Many large cartilaginous portions of bone, which were not yet perfectly softened, were removed with the knife by degrees, and the walls of this great cavity were then cauterized. The hæmorrhage quickly ceased, and I then united the flaps of the face; in all, twenty sutures were employed. The patient was still able to stand after the operation; he was placed in bed, and took a drink of wine and water. His treatment was moderately cooling. As his strength was more sunk next day, I gave him an infusion of valerian, but I was soon obliged again to have recourse to saturated solutions of an alkali, Selter's water, and to open his bowels with salts. On the fourth day, the entire wound in the face was healed, and all the sutures were withdrawn. At the inner angle of the eye a penetrating opening formed, caused by the thinness of the skin; this was easily healed. By gently nourishing and strengthening treatment, the patient made so much progress towards recovery, that twenty-eight days after the operation he was able to put on all his clothes, and to walk about his chamber. He had left his bed at this time fourteen days.

CASE XVIII. A lady, æt. 60, had perceived, a long time ago, after the spontaneous falling out of her back teeth, a tumour on the right side of the under jaw, which increased, until it became of the size of a fist. Swallowing, speaking, and breathing was much impeded, and life was endangered, as the tumour filled

the greater part of the mouth ; above, it was unattached, and below it was fixed between the internal and external lamellæ of the lower jaw, which it had forced widely asunder.

In extirpating this tumour with the knife, I found the extreme laxity of the lips a very favourable circumstance, as on account of it I was not obliged to split the mouth. In performing the operation, the tumour was drawn forward with a hooked forceps, and dissected out with a knife.

When this was finished, I sawed off the projecting edges of the lower jaw, which was absorbed in the centre. This healed up in a few weeks, but a little while after a splinter of bone was thrown off.

This tumour was fibrous, and on its anterior and even surface was the sac of an hydatid, filled with a clear fluid like the white of egg.

In the majority of the cases of osteosarcoma here related, I should formerly have applied internal and external treatment ; yet such was never of any avail, and the disease generally proceeded through months of treatment regularly to increase. Mercury, iodine, gold, and the decoction of Zitmann were the principal remedial agents which I employed to combat this disease, and externally the acetate of lead, which certainly sometimes caused a pause in the course of the disease.

In the greater number of patients, the fungus commenced on a single alveolus, and then, in place of attacking the alveoli on both sides, engaged only one of the neighbouring alveoli, and then the next to that again, &c. It was rare to see the disease spread equally on both sides at the same time. Drawing the teeth promoted the quicker development of the disease. I do not say this from actual observation, but the patients always complained of it.

When the alveoli to a large extent were diseased, then the os zygomaticum usually was engaged before the palate bones. The whole cheek formed an oval hemisphere ; the nose was

pushed over to the far side, and the cheek of the diseased side was no longer separated from the tumour.

I have seen a great number of such patients die, not only in La Charité, but also in private practice, even when tolerably well treated, even till death. In some, the fungus broke through the thinned skin, which had hitherto been of a brownish red colour, and was to be seen with its red fungous tissue laid bare ; in others, a sinus formed in the cheek, from which foetid matter exuded. The fungus then protruded from its walls ; the cavities of the nostrils became filled with a fungous mass, and the patient breathed through the mouth.

When a seton was drawn through the tumour before the disease had reached its height, it appeared to cause its quicker development, and the patients died sooner. This melancholy experience it was, which compelled me to undertake excision of the bones of the face in a more extended degree than I had before, and I was encouraged by the success attending operations on a smaller scale. As a general conclusion, relapses occurred seldomer after excision than after operations for cancer or fungus in other parts, and certainly less seldom than after the removal of cancer of the breast. I prefer the decoction of Zitmann to all other remedies as a preventive against relapse.

In conclusion, M. Dieffenbach states, that the method he has adopted, of severing the face in the mesial line, cannot be uninteresting in the history of surgery, as by means of it, the duct of Steno remains uninjured, and the paralysis which must follow division of the cheek far back is totally avoided.

S. L. L. BIGGER.

ART. III.—*Case of Primary Laryngo-Tracheitis (with False Membrane) in the Adult.* By MARTIN H. LYNCH, M. D., of Newcastle-upon-Tyne, Lecturer on the Practice of Medicine and Medical Jurisprudence, &c. ; and WILLIAM DAWSON, M. R. C. S. L., Lecturer on Midwifery and the Diseases of Women and Children.

MRS. H., aged 28 years, in the eighth month of pregnancy, having been engaged in washing a very warm room on Wednesday, the 9th of May, and having, while heated, imprudently exposed herself to a current of cold air, attributed to this circumstance some slight bronchitic symptoms which appeared on the following day, and continued for about forty-eight hours, when there were superadded uneasiness, almost amounting to pain, in the anterior part of the neck, sibilous respiration, great dyspnoea, high fever, &c. We were not called upon till the 15th. Mrs. H. had been, up to our visit, under the care of a druggist, who had merely administered a purgative, and applied a few leeches to the throat.

May 15th, 11 o'clock, A. M. Skin hot ; countenance pale, but tumid and anxious ; thirst, (cold water being preferred ;) bowels not freed since the day before yesterday ; tongue moist, furred ; region of the larynx painful on pressure ; deglutition very difficult ; pulse 124, hard, and resisting, although small ; great dyspnoea ; whispering voice ; sibilous and difficult inspiration ; cough, which sometimes occurs in paroxysms, when it is "barking," (to use the expression of her mother, who is in attendance upon her ;) darting pain on the right side of the chest since morning ; expectoration viscid, of a yellowish green ; her mother states that before we had seen her, she had coughed up a tough flat substance of an irregular form like a piece of skin ; internal fauces and pharynx in a natural state.

On examining the chest we found the sound, on percussion,

clear ; *no abnormal sounds* were heard on applying the stethoscope, but it was equally impossible to hear the murmur of respiration, in consequence of the loudness of the laryngeal rale, and probably on account of the weakness of respiration, resulting from the obstruction in the glottis. The pulling forward of the tongue *was not productive of pain*. We endeavoured to get a view of the epiglottis, but failed, the attempt always giving rise to a paroxysm of dyspnœa and coughing ; we found that her mother's description of the character of the cough during the paroxysms was perfectly correct, it was "barking," and at the same time ringing, and clangous.

The treatment consisted in the application of leeches to the region of the larynx ; the administration of purgative enemata ; the exhibition of the following solution of tartarized antimony, to be given in half ounce doses, so as to maintain nausea, and excite moderate vomiting.

℞ Antim. Tart. gr. iii. Aq. Distill. ℥ viii. solve.

Mercury was ordered by way of inunction into the axilla, and a large blister to be applied, *inter scapulas*, and to extend to the nucha. A draught containing five drops of Battley's sedative solution to be given every three hours.

The patient was visited at intervals of three hours at the most ; there was nothing new observed until three o'clock, except that the signs of foetal life had disappeared ; at half-past three o'clock she stated that her breathing was performed with more ease, especially after vomiting ; turns in the bed without assistance or much difficulty ; expectoration copious, paroxysms of threatened suffocation fewer ; the bowels have been once freely moved.

Repetatur enema ; continuentur medicamenta cætera.

This improvement continued until four o'clock, A. M., on the 16th, when suddenly she was attacked with paroxysms of dyspnœa of unusual severity, which quickly recurred until she expired at 7 A. M.

Necropsia, twenty-nine Hours after Death.—The neck was very short ; when the trachea was exposed, we were struck with its very small calibre ; indeed it might, in this respect, be compared to that of a child.

The mucous membrane covering the epiglottis was vascular, especially towards the base, and was covered posteriorly, i. e. on its laryngeal surface, with a yellow, viscid, puriform matter ; the epiglottis was erect, hard, and wonderfully thickened, representing one-fourth of a *sphere* instead of a *plane*.

The larynx and trachea were split open posteriorly along the median line, the internal surface of the larynx was covered with a viscid, ochre-coloured, puriform matter. The trachea was lined with a buff-coloured membrane, nearly as thick as the rings of the trachea, and extending to the bifurcation of the bronchi ; it formed a perfect cylinder, except at one point posteriorly, and at the lower part of the trachea, where there was a longitudinal deficiency : the cylinder of false membrane was free inferiorly, it was adherent above.

The larynx was lined with a thin filmy continuation of the tracheal false membrane ; this filmy membrane was shreddy at some points, through which it seemed to give exit to the ochre-coloured matter. The mucous membrane of the trachea, where the detachment of the false membrane left it visible, was found highly vascular. The folds of the membrane forming the edges of the glottis, were hard and swollen, the ventricles of the larynx were hardly visible, being nearly obliterated by the inflammatory thickening and exudation.

The lungs exhibited sanguineous congestion even at the superior part, but were soft, crepitating, and the blood was easily expressed. The bronchial tubes exhibited an inflammatory injection below the termination of the false membrane, but on following up their dissection, we perceived that the vascularity gradually disappeared as we approached the smaller tubes, and ultimately disappeared altogether, the mucous membrane being in a perfectly natural state. In ascertaining the existence of

vascularity, we did not omit careful washing in order to distinguish between inflammatory injection, and mere colouring produced by the blood effused during dissection.

No appearances were found in any other part of the body sufficient to account for death.

The larynx, trachea, and as much of the pharynx and fauces, as a hurried dissection allowed us to remove, have been carefully preserved by one of us, (Mr. Dawson,) and the preparation has been placed in the museum of the Newcastle-upon-Tyne School of Medicine and Surgery.

Remarks.—This case was clearly one of simple, uncomplicated inflammation of the larynx and trachea: during life we ascertained that the fauces and pharynx were quite free from disease, a fact confirmed by dissection.

The distinction between uncomplicated or *primary* croup, and *secondary* croup, or that which is consecutive upon affections of the fauces and pharynx, is one of great importance, inasmuch as the former is always attended with sthenic symptoms, but the latter generally with symptoms of an asthenic character. The credit of making this division, is due to Dr. Copland and Dr. Stokes. (Copland's Dictionary, 1835, Art. Croup; and Stokes on Diseases of the Chest, Dublin, 1838.)

That *primary* croup, leading to the formation of a false membrane, is an extremely rare disease in the adult, may be inferred from the fact, that Dr. Stokes, notwithstanding his great talent, experience, and research, has been able to bring forward but one case of it. This has been quoted from Louis, *Memoires Anatomico-Pathologiques*.

The circumstance of no pain being produced by drawing forward the tongue, seemed to point out that the epiglottis was not much affected; nevertheless, dissection shewed that it had been intensely inflamed.

Although we endeavoured to avail ourselves of every re-

mission of the symptoms, the use of the stethoscope led to no results whether we applied it to the thorax or over the trachea.

We consulted upon the propriety of performing the operation of tracheotomy, but determined against its expediency. First, because without any *leading question* having been asked, we had been told, that some false membrane had been coughed up. Secondly, because the cough was very different from that of pure laryngitis, being, on the contrary, like that of croup, clangous, ringing, and as it were metallic.

We are of opinion, that in this case the nature of the cough would have been, alone, sufficiently diagnostic to a practitioner, who had seen several patients affected with each disease, croup and mere laryngitis. This cough is so peculiar, that no words can accurately describe it. Dr. Cheyne, however, long since observed, that the cough of cynanche trachealis cannot be confounded with that of laryngitis.

It is to be desired, that this symptom could be calculated upon as a sure diagnostic sign in all instances, for, notwithstanding the absence of stethoscopic inflammation, it would then enable us, in cases of inflamed windpipe in the adult, to determine that the trachea is affected, and the likelihood of its containing a false membrane, and would thus prevent us from performing a worse than useless operation.

It can hardly be doubted, that some advances had been made towards resolution of the inflammation during the improvement of the symptoms, which lasted for several hours, when a diminution of the dyspnœa, and increase of expectoration occurred. During this time, probably, the false membrane became extensively detached inferiorly.

Did this detachment of the membranous cylinder inferiorly produce asphyxia, by allowing it to flap upwards and plug the trachea, thereby rendering *expiration* incomplete or impossible, at a time when the resolution of the inflammation was in the act of removing or diminishing the obstruction to inspiration?

ART. IV.—*Researches on Operative Midwifery*. No. I.
Induction of Premature Labour. By FLEETWOOD CHURCHILL, M. D., Physician to the Western Lying-in Hospital, and Lecturer on Midwifery, &c. in the Richmond Hospital School.

[Read before the Surgical Society of Ireland, April 7, 1838.]

“ Neque temerè neque timidè.”

MR. PRESIDENT,

I beg leave to call your attention, and that of this learned society, to an operation of great value in certain cases, in the hope, that from the experience of so many of the profession, a greater amount of accurate information may be acquired than we at present possess. I allude to the induction of premature labour, for the purpose of saving the life of the infant, of its mother, or of both. It is of comparatively modern origin, and is one of the few instances of an improved science augmenting the number of operations.

There would appear to be, in the minds of all men, a repugnance to interfere with the natural progress of those great phenomena which ordinarily run a definite and uniform course, and in the present instance this objection is increased, because the supposed interference is to remedy an irregularity. Accordingly, the first consideration has always been, not the *usefulness* but the *morality* of the operation. Dr. Denman states,* that Dr. Kelly informed him, “ that about the year 1756, there was a consultation of the most eminent men at that time in London, to consider of the *moral rectitude* of, and the advantages which might be expected from, this practice, which met with their general approbation.” The conclave decided in favour of the morality of such interference, and shortly afterwards,

* Introduction to Midwifery, p. 318, 7th Ed.

the operation was successfully performed by Dr. Macauley. Subsequently, Dr. Kelly “ practised it, and among other instances, he mentioned that he had performed this operation three times upon the same woman, and that twice the children had been born living.”* So numerous, and upon the whole so successful have been the instances in which it has been tried since Denman’s time, that it has taken its place among the regular obstetric operations in the various systems of British writers and teachers, and among its supporters may be found the distinguished names of Denman, John Clarke,† John and James Barlow,‡ Burns, Merriman,§ Conquest,|| Gooch,¶ Blundell,** Hamilton,†† &c. Dr. Denman’s remarks are so much to the point, that I may be excused if I quote them. “ With regard to the morality of the practice, the principle being commendable, that of making an effort to preserve the life of a child, which must otherwise be lost, and nothing being done in the operation which could be injurious or dangerous to the mother, but on the contrary, a probability of lessening both her danger and suffering, I apprehend, if there be a reasonable prospect of success, no argument can be adduced against it, which will not apply, with equal force, against any kind of assistance at the time of parturition, against inoculation, or medicine in general, and, in fact, against the interposition of human reason and faculties in all the affairs of life.”§§

In France, however, the proposed operation was by no means so frankly received, or so readily adopted. Certain doc-

* Introduction to Midwifery, p. 319.

† London Practice of Midwifery, p. 233, 6th Ed.

‡ Med. and Phys. Journal, vol. v. p. 40. Med. Facts and Observations, vol. viii.

§ Med. Chir. Trans. vol. iii. p. 127. Synopsis of Difficult Parturition, p. 179.

|| Outlines of Midwifery p. 135. ¶ Lectures reported by Skinner, p. 221.

** Principles and Practice of Obstetricy, p. 559.

†† Practical Observations, Part II. p. 173.

§§ Introduction to Midwifery, p. 319.

trines of the national church, or at least of the interpretation of them by the Doctors of the Sorbonne, touching the importance of foetal life, seems to have aggravated the risk of the operation, and to have deterred professional men from making the attempt. The great name and extended influence of Baudelocque* were opposed to what he considered (in the case supposed) a crime; and a teacher of the present day, Capuron,† has stigmatized it as “un attentat commis envers les lois divines et humaines.” Even so late as 1827, on the occasion of a memoir presented by M. Coste, demanding if it would be allowable to bring on labour prematurely in females labouring under aneurism of the heart, the Academie Royale de Medicine pronounced the question “inconvenient et presque immorale.”‡ It is said, however, by M. Sue,§ that M. Petit ranged himself on the side of the advocates of the operation, and in latter times, it has been recommended and practised by Velpeau,|| Stolz,¶ Paul Du-bois,** Dezeimeris,†† Burckhardt,‡‡ and Figueira.§§

The objections may be thus summed up :

1. It is immoral. (*Sue, Capuron, Baudelocque.*)
2. It is almost impossible to determine the exact relations between the head of the child and the pelvis. (*Capuron, Baudelocque, Osiander.*)
3. The danger of the manœuvres necessary for exciting labour. (*Capuron.*)

* De l'Art des Accouchemens, vol. ii. p. 285.

† Traité Theorique et Pratique sur l'Accouchemens, p. 538.

‡ Marinus on Induction of Premature Labour, in Bulletin Med. Belge, Sept. 1837.

§ Essais Historiques sur l'Art des Accouchemens, vol. i. p. 606.

|| Traité Complet de l'Art des Accouchemens.

¶ Memoire présentée a l'Acad. Royale de Med. 1833.

** Thèse du Concours, pour la chair de l'Accouchement, 1834.

†† Art. *Accouch. Premature*, Dict. de Med. in 21 vols.

‡‡ Essai sur l'Accouchement Premat. Artif. Thèse, Strasburgh, 1830.

§§ Etude de l'Accouchement Premat. Artif. &c. Thèse soutenue a Montpellier, 1837.

4. The uncertainty of all women as to the period of their pregnancy. (*Baudelocque, Osiander.*)

5. The difficulty of dilatation of the os uteri at the seventh month. (*Baudelocque.*)

It is quite evident, as M. Marinus observes, that these writers had in view the “accouchement forcée,” performed at the seventh or eighth month; a widely different operation, and one perfectly unjustifiable at so early a period.

It has been adopted in Germany by Ant. May, Weidmann, C. Wenzel, Mende, Bettcheler, Kluge, Siebold, Burck,* Reisinger, Froriep,† &c.; but opposed by Stein, Osiander,‡ Joerg, &c. In Italy, it seems to have met with less opposition, or at least less aversion has been expressed. Successful cases have been published by MM. Ferrario,§ Billi, and Lovati.|| Paul Scheel,¶ in Denmark; Solomon de Leyden, and Professor Vrolek in Holland; and M. Marinus in Belgium, have each advocated the practice.

* Handbuch der Entbindungskunst.

† Handbuch der Geburtshulfe, p. 499.

‡ Osiander objects, 1st, that children at the 28th or 32nd week, generally die at birth, or soon after, and that the calculation of pregnancy is very uncertain, rendering the induction of premature labour of no more value, in some cases, than perforation. 2nd. That it is impossible, before labour, to say, that the relative diameters of the foetal head and the pelvis are such, that the foetus cannot be transmitted at nine months. 3rd. That the situation of the os uteri renders the operation difficult. 4th. That spasm of the orifice or neck of the womb may be an impediment. 5th. There is danger of wounding the soft parts of the mother. 6th. Or of injuring the presenting part of the child. 7th. Labour may not come on after evacuation of the liq. amnii. 8th. The child may be dead. 9th. Even if living, artificial assistance may be required to complete the delivery. 10th. When born alive, the child seldom lived. 11th. There is more danger of hæmorrhage than in ordinary labour.—*Handbuch der Entbindungskunst*, vol. ii. p. 241; vol. iii. p. 631.

§ Journal Compl. des Sci. Med. vol. xxxiv. p. 359.

|| Annali Univ. di Medicina, Trans. by M. Ollivier, in Archives Gener. de Med. Jan. 1833.

¶ Dict. de Med. vol. i. Art. Accouch. Premat. (Dezeimeris.)

So much for the history of this operation, and the difficulties attendant upon its introduction into practice. As to the origin of it, all writers are agreed in attributing it to the following circumstances: it has not unfrequently happened that the life of a seventh or eighth months' child has been preserved by accidental premature labour, in cases where the birth of a child at the full term had been previously found impossible, from pelvic distortion. From the complete success of such cases, as regards both mother and child, it was inferred, that premature labour, artificially induced, might, in certain cases of pelvic deformity, be employed to supersede an operation (craniotomy) which involved not only the destruction of the child, but considerable risk to the mother. The proposal was not, it must be remembered, to deliver the foetus artificially, but merely, as was stated by Ritzen, "to communicate a slight but certain impulse,"* by virtue of which the process of parturition may be carried on and completed by the natural powers.

The reasoning of Dr. Denman appears to me conclusive, as to the "moral rectitude" of the operation: the next question, therefore, is, as to its safety to the child and the mother, confining ourselves for a moment to the consideration of the cases originally proposed to be benefited by the operation. It is pretty well established, that a foetus is "*viable*" at the completion of seven months of utero-gestation, and many instances are on record of children born at that period living to a good old age. M. Chaussin (of Dijon) and his wife were both seven months' children: his Majesty George III. was also a seven months' child; and M. Foderè relates the case of the wife of a judge whose pregnancies always terminated at the seventh month. Examples of "*viable*" infants born at an earlier period are likewise to be found: but I shall at once refer the Society to the able work of my friend, Dr. Montgomery, for further

* In Gemeinsame deutsche Zeitschrift für Geburtshülfe.

details ; concluding from all the evidence we possess of the viability of seven months' children, that premature labour, accidentally or artificially induced, at the completion of the seventh month, does not involve danger to the child from the immaturity of its growth merely.

As to the actual risk of labour to the foetus, as ascertained by an estimate of facts, I may adduce the following testimony ; of twelve cases mentioned by Denman, the majority of the children were saved. Mr. Barlow* reports seventeen cases ; six children were still-born, five died a few hours after birth, and six lived. Of Dr. Merriman, Sen. ten cases ; four children were saved. Dr. Merriman mentions forty-six cases ; sixteen children lived, and all the mothers recovered. Dr. Conquest says, that out of nearly 100 cases, about half the children were born alive.† According to M. Velpeau, 115 children were saved out of 161 cases. M. Figueira collected 280 cases from different sources, of which 166 children were saved.

Dr. Hamilton states, that “ previous to the 26th of January, 1836, the author brought on premature labour in twenty-one individuals, on account of defective apertures, viz. in fourteen once ; in one, twice ; in three, thrice ; in two, four times ; and in one ten times. Of the forty-five infants thus prematurely brought into the world, forty-one were born alive. The death of the four still-born can be readily accounted for.” “ In the practice of Mr. Moir, and Dr. John Moir, premature labour was induced twelve times on six women. Nine of the infants were born alive, and the cause of the death of the three still-born infants could not be attributed to the operation.”‡

I think we may conclude from these different data, that more than half the children were saved, notwithstanding a cause of failure to which I have not yet referred. I allude to the greater

* Med. Facts and Observations, vol. viii.

† Outlines of Midwifery, p. 136.

‡ Practical Observations, Part II. pp. 180-182.

frequency of mal-presentations in premature labour, than in labour at the full time.* In Dr. S. Merriman's cases, for example, there were eighteen mal-presentations out of the forty-six, only one of which was saved. If we could subtract all the cases of mal-presentations, we should find, I doubt not, that the proportion of children lost, to those saved by the operation, was very much smaller.

There is unquestionably some risk incurred by the mother, but not more than an accidental premature labour. After much consideration Denman concludes, that "it is perfectly safe to the person on whom it is performed."† We have already seen that Dr. Kelly performed it three times successfully on one person. Dr. S. Merriman seems to think, that its safety was rather overrated, but he adds, "at all events, the method in question, if carefully conducted, cannot be more hazardous to the mother, perhaps is much less so, than the operation for lessening the head of the foetus in utero, and it is incomparably less perilous than the Cæsarian operation, or the division of the symphysis pubis."‡ Out of his forty-six cases, not one proved fatal!

Dr. Hamilton observes, "The late Dr. Merriman first called in question the safety of the operation, but the cases on which he formed his doubts on this point, were evidently cases of accidental coincidence, for the safety of the practice is now fully established."§

Dr. Blundell concludes his observations by saying, that "with all its faults about it, the practice is of great value, and there are now living in society, individuals whose heads have, in this manner, been preserved from the perforator."||

The statistical details given by Velpeau and Figueira, would

* Blundell's *Principles and Practice of Obstetricy*, p. 561.

† *Introduction to Midwifery*, p. 319.

‡ *Med. Chir. Trans.* vol. v. p. 134.

§ *Practical Observations*, Part II. p. 174.

|| *Principles and Practice of Obstetricy*, p. 161.

justify I think, a much more unqualified commendation. Velpeau found that it has been performed

In Great Britain	.	.	.	72 times.
In Germany	.	.	.	79 „
In Italy	.	.	.	7 „
In Holland	.	.	.	3 „

Making a total of 161 cases, of which number, eight mothers died, five of them, however, from causes unconnected with parturition.

M. Figueira has collected 280 cases, of which only six mothers died. We may therefore conclude, with M. Merriman,* that “if these facts be true, it is established, that females undergoing this operation incur no immediate danger, and if we push our researches still farther, we shall find that these same females were not attacked by pure lesions of the uterus, as has been advanced; several of them underwent the operation two or three times, with as much safety as if they had been delivered at the full term of utero-gestation.”

We have now only to inquire as to the *utility* of the operation, before considering the cases to which it is applicable.

It is peculiar to midwifery operations, that they form ascending series, increasing in gravity from the simplest to the most severe, no two being equal; and therefore, in considering the suitability or practicability of any one, we do so with the knowledge, that if the one we prefer does not succeed, we must have recourse to another more severe and more dangerous. An example will make my meaning clear. If, in any given case, we attempt to deliver with the forceps, but are not able to succeed, we must subsequently have recourse to the perforator; there is no other method of only *equal* severity with the forceps which we can try. Or again, if craniotomy and evisceration

* Bulletin Med. Belge, Sept. 1837, p. 1.

will not render the transit of the child possible, we have no resource but symphyseotomy or Cæsarean section.

Thus, the *alternative* of any operation in midwifery, is not one of *less*, or even of *equal* danger, but *necessarily* one of a *more serious nature*, and consequently we cannot estimate the utility of any obstetric operation fairly, if we consider it by itself; a just appreciation involves a due estimate of its alternatives.

It is to the *alternatives* of the induction of premature labour, that I would wish to call the attention of the Society, as demonstrating very strikingly the *utility* of the practice, in addition to the positive facts I have already brought forward.

In the cases which have been supposed to demand this operation, there is always a considerable diminution in the calibre of the pelvis from bony distortion, and it would therefore be quite useless, at the full term of utero-gestation, to attempt the delivery by the forceps; the only *alternatives*, if we allow pregnancy to be completed, are, the perforator, symphyseotomy, and Cæsarean section.

Now let us compare the mortality attendant upon each of these operations with the results of artificial premature labour.

1. By the use of the *perforator*, not only are all the children destroyed, but extensive statistics have shewn, that about one in five of the mothers perish, either from the direct effects of the operation, or from the length of the previous labour.

2. *Symphyseotomy*, or the sigaultian operation, has never obtained any estimation in this country; it has once, I believe, been practised with such fearful results, that it will probably never be repeated. It is a very severe and most unscientific operation, involving the loosening or separation of all the joints of the pelvis, and months of subsequent suffering, without always facilitating delivery. It has been frequently performed on the Continent, and it may be useful to refer to the results.

M. Deges reports forty-one cases, of which fourteen women and twenty-three children were lost; M. Velpeau enumerates

forty-four cases, fourteen of which died; and M. Figueira 157 cases, of which seventy-two women died. Barlow, Capuron, and Velpeau state that the child rarely survives.

3. *Cæsarean section* is the "*dernier ressort*" of midwifery, involving the utmost danger to the mother and child, and justifiable only when no other chance for either remains.

Baudelocque has collected 73 cases, out of which 42 mothers

Michaelis	„	„	110	„	„	62	[died.
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Dubois	„	„	160	„	„	100	
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Figueira	„	„	790	„	„	424	
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Thus more than half the mothers were lost, and of the children very few indeed are saved.

If then to the *absolute* advantages of the operation proposed, be added the *comparative* gain from avoiding these terrible *alternative* operations, we may form a tolerably correct estimate of the *utility* of the "induction of premature labour."

Having, as I trust, established, from facts and testimony, the three leading principles of the *mortality*, *safety*, and *utility* of this operation, I shall now proceed to inquire as to the cases in which it is available.

1. The class of cases, for which it was first proposed, and in which it has been most frequently employed, is that in which the diameters of the upper outlet of the pelvis are too much reduced by distortion to permit the passage of a foetus at the full term, and yet not so much diminished as to prohibit the passage of a foetus at an earlier but still "*viable*" age. In the words of Denman: "it is under circumstances and in situations preventing the successful use of the vectis or forceps, and just compelling us to the fatal measure of lessening the head of the child, that it may be a duty to propose on a future occasion, the bringing on of premature labour."

The first step is to endeavour to ascertain the size of the foetal head at different periods of utero-gestation. After the seventh month, in order that by adapting the diameters of the

deformed pelvis to the appropriate diameters of the foetal cranium, we may be enabled to fix upon the moment when they are in correspondence, for the induction of premature labour.* It is of course impossible to do this in any individual case, but an approximation may be attempted by taking the measurements in a considerable number of cases at the same periods.

The following table has been thus constructed by M Figueira.

Age of Fœtus.	Bi-parietal Diameter.	Occipito-parietal Diameter.	Occipito-bregmatic Diameter.
7th Month.	2 in. 9 lines.	3 in. 8 lines.	2 in. 10 lines.
7½ „	3 „ „	3 „ 9 „	3 „ „
8th „	3 „ 1 „	3 „ 10 „	3 „ 1 „
8½ „	3 „ 2 „	4 „ „	3 „ 2 „
9th „	3 „ 4 „	4 „ „	3 „ 4 „

To this kind of calculation it has been objected, that we cannot be quite sure of the exact age of the fœtuses measured, and to the practical use of it, that the female cannot be quite sure of the exact period of pregnancy, (*Blundell*.)† That this objection has a certain weight, must be admitted, but that it is sufficient to prohibit the operation, I cannot believe, for, it may always be obviated in practice, by assuming the longest possible period of pregnancy. If for example, a patient imagine that she is six months pregnant, but that she may be six and a half, by calculating for the six and a half months we shall have assumed the largest size to which the fœtal head can have attained, and if labour be not brought on till seven

* D'abord comment s'assurer de l'époque où il faut provoquer l'accouchement prématuré, puisque la tête étant incommensurable dans la matrice, il est impossible d'en assigner le rapport avec le bassin?"—*Capuron Cours Theorique et Pratique sur l'Accouchement*, p. 538.

† Principles and Practice of Obstetricky, p. 561.

months and a half, we shall also have secured a foetus of the “*viable*” age.

Ritzen has made another series of calculations, which have led to the following practical adaptations.

He says that labour may be induced

At the 29th week, when the antero-posterior diameter of the pelvis is 2 in. 7 lines.

„ 30th	„	„	„	„	2 „ 8 „
„ 31st	„	„	„	„	2 „ 9 „
„ 35th	„	„	„	„	2 „ 10 „
„ 36th	„	„	„	„	2 „ 11 „
„ 37th	„	„	„	„	3 „ 0 „

There is a very slight difference between the tables of Figueira and Ritzen, which may be allowed for in practice. The compression of the foetal head will also render its diameter less than the subsequent measurement would lead us to suppose.

It will be at once observed, that there are two measurements of the pelvis which limit the operation; if the pelvis exceed the greater measurement, the operation is uncalled for; and if less than the least, it will not succeed in saving the child. The smallest of these diameters appears to be about two and a half inches, and the greater three and a quarter, (*Busch*.) If the pelvis in its sacro-pubic diameter, be less than the former, a “*viable*” child will not pass, and it is generally admitted that a living child may be propelled through a pelvis whose antero-posterior diameter is three and a half inches. The opinions of different authors accord with this calculation. Dr. John Clarke* says “that the time may be bounded on one side by seven months, and on the other by eight and a half.” “When the distance between the pubes and sacrum is under three, yet all but three inches, eight months may be allowed; when the distance is two and three quarters, seven months; and so on. Mr. James Barlow,† observes, “I presume then, that a pelvis,

* London Practice of Midwifery, p. 235.

† Medical and Physical Journal, vol. v. p. 46.

the small diameter of which measures from pubis to sacrum, about two inches, or two and a half inches, appears to favour the success of this operation more than any other dimensions." Dr. Gooch* recommends us "to reckon seven calendar months and one week from the last menstrual discharge, and then bring on premature labour." Dr. Blundell† names seven months and a fortnight, if our object be to save the child. With great confidence, therefore, the author can recommend this practice, in all cases where the deficiency of space in the apertures of the pelvis, does not fall under two inches and a half. (Hamilton.‡)

M. Figueira observes, that "every time that the sacro-pubic diameter is from $2\frac{1}{2}$ to $3\frac{1}{4}$ inches, premature delivery is indicated from 7 to $8\frac{1}{2}$ months."

M. Marinus advocates the end of the seventh month, when the pelvis is $2\frac{1}{2}$ inches in its sacro-pubic diameter.

M. Busch fixes upon $2\frac{3}{4}$ inches as the medium diameter, and the twenty-eighth week as the best period for the operation.

Another difficulty still remains, which has been put forward as a very serious objection by the opponents of this operation; and this is the uncertainty of ascertaining the exact diameters of the pelvis in the living subject. Various mechanical contrivances have been proposed by Aitken, Coutouly, Baudelocque, Asdrubali, Chaussier, and others, (pelvimeters as they are called,) into the merits of which I do not propose to enter at present; it is sufficient to say, that in this country they could rarely or never be employed. Nor do I think them necessary; a well practised finger is after all the best pelvimeter, and will yield sufficiently accurate information. But giving the utmost

* Lectures edited by Mr. Skinner, p. 222.

† Principles and Practice of Obstetricy, p. 560.

‡ Practical Observations, Part II. p. 183.

force to this objection, to what does it amount, as Velpeau justly observes: "if the pelvis be wider than we thought, premature delivery (at or after the seventh month) is accomplished without risk. If, on the contrary, the narrowing be more considerable, the foetus will certainly perish, but then had no operation been attempted until the full term, the foetus would equally have been lost, and the mother would have run greater risk." Besides much information may be derived from the history of the previous labour of the patient, for it is rarely or never for the first child, that the induction of premature labour is proposed. Dr. Merriman* remarks, "that the use of the perforator in a former labour, is not *alone* to be considered as a justification of this operation." This is undoubtedly true in the present uncertain state of opinion, concerning the use of the forceps and crotchet, inasmuch as the latter instrument is frequently used where there is no distortion.

But if we are convinced that the perforator was used from the impossibility of otherwise delivering the patient, it might then be an adequate reason, and if it further appeared that her labour had been thus terminated more than once, and for the same reason, the operation would then seem to be imperatively required.—(*Blundell*.†)

2nd. It is possible that a narrowing of the bis-ischiatic diameter of the lower outlet, or a morbid growth, (fibrous or osseous tumours,) offering a fixed impediment to parturition, might become an equally valid ground for the induction of premature labour.—(*Denman*.‡)

3rd. In the cases I have supposed, the safety of the child is the great object of the operation, and they are limited therefore to those patients, in whom the pelvis, though deformed, is still large enough to permit the passage of a "*viable*" child.

* Synopsis of Difficult Parturition, p. 183, *fourth edition*.

† Principles and Practice of Obstetrics, p. 559.

‡ Introduction to Midwifery, p. 321.

But there are cases where the distortion is so great as to render the passage of a seven months' child impossible, and others still worse, when no reduction of the child's bulk will enable it to pass. I do not see why abortion should not be induced at an early period in such cases. The life of the child must inevitably be sacrificed, and the safety of the mother alone regarded; and surely after the calculations I have adduced,* it cannot be pretended that Cæsarean section, the *alternative* in these cases, offers an equal chance to the mother, or such a chance to the child as would justify our preferring it.†

An objection to this extension of the operation has been made by Dr. Merriman and others, on the score that it would be "opening a wide door to the dreadful abuse of the operation." That in short, by multiplying the examples of inducing premature labour or abortion, we should run the risk of its being performed unnecessarily or for wicked purposes. But so may the fact of its being performed at all, and so may the practice of using ergot of rye for the purpose of exciting uterine contractions. I do not, in truth, see much force in this objection, nor do I anticipate any such prostitution of their power on the part of the members of our profession, and beyond the profession the operation is not likely to be much known. It will of course be necessary, that the case be thoroughly investigated by more than one person, and the time appropriately chosen.

* See page 48.

† Mr. Barlow (Med. and Phys. Jour. vol. v. p. 51,) states a curious question on this point, "as to how far the morality of the practice may be justifiable when performed with the view of preserving the life of the mother, and sacrificing that of the foetus." "In such a situation is the accoucheur excusable, who tacitly complies with the requests of the mother, and voluntarily sacrifices a number of immature foetuses, with a view to her own preservation?" In other words, Mr. Barlow thinks, that if the woman go on conceiving, and we operating, the *value* of the foetuses destroyed may equal that of the mother's life.

4th. In certain cases of rupture of the uterus, the cause is almost entirely mechanical. There is some narrowing of the upper outlet, perhaps a projection of the promontory of the sacrum, offering an obstacle to the ready descent of the foetal head, which is driven forward with great force by the uterine contractions. Under such circumstances, the head may be pushed to one side, and if the tissues are not very firm, it will be driven through them into the cavity of the peritoneum. Recovery from such an accident is very rare, but nevertheless it has occurred, and if the woman become pregnant subsequently, a premature delivery may serve both mother and child. As the best argument I can employ in favour of this operation in such cases, I may mention that it was adopted successfully by the late distinguished master of the Great Britain-street Lying-in Hospital (Dr. Collins.) The patient had recovered from rupture of the uterus and became pregnant. She was admitted into the hospital in the seventh month of pregnancy, and the membranes were ruptured on the 4th of March, 1832. Labour came on on the seventh, and was completed in ten hours. The patient was delivered of a living child and recovered. The child, however, lived but two days. The case is perfectly illustrative of the advantages which may be derived from the operation in this class of cases. The mother was saved, and the child at birth appeared likely to live; its death does not seem to have resulted either from its early age, or from the labour.*

5th. Dr. Denman† observes, “there is another situation in which I have proposed and tried with success, the method of bringing on premature labour. Some women who readily conceive, proceed regularly in their pregnancy, till they approach their full period, when without any apparently adequate cause,

* The details of the case will be found in Dr. Collins's “*Practical Midwifery*,” p. 255.

† Introduction to *Midwifery*, p. 321.

they have been repeatedly seized with rigor, and the child has instantly died, though it may not have been expelled for some weeks afterwards. In two cases of this kind, I have proposed to bring on premature labour when I was certain the child was living, and have succeeded in preserving the children without hazard to the mothers. There is always something of doubt in these cases, whether the child might not have been preserved without the operation, but as such cases often come under consideration, and as I am disclosing all that my experience has taught me, it seemed necessary to mention this circumstance." Mr. Barlow* thinks the "doubt" expressed in the above extract, a sufficient ground for negating the operation. Whether it be so or not, I am not prepared to say; on such a point the opinion of the many experienced men who are members of this society would be most valuable.

6th. The question has been quoted, whether it would be right to induce premature labour on account of the presence of certain diseases caused by, or connected with pregnancy. Denman remarks: "The propriety of this practice has also been considered when women have, during pregnancy, suffered more than common degrees of irritation, and especially when the stomach is in such a state, that it cannot bear nourishment of any kind or in any quantity, and the patients are thereby reduced to a state of dangerous weakness. Presuming that these symptoms are purely in consequence of pregnancy, it may, perhaps, be justifiable to bring on premature labour." Dr. Merriman† relates a case occurring in the practice of a "provincial surgeon of considerable eminence." "She was teased with a very severe cough, and her stomach was so irritable as to retain no food whatsoever, nor even opium in a solid form. She had taken absorbents, stomachics, bitters, aromatics, and opiates, without experiencing any relief: liniments, fomentations, and blisters, had been extensively applied without benefit, and she

* Med. and Phys. Jour. vol. v. p. 52.

† Med. Chir. Trans.

was thought to be sinking into her grave, when it was proposed, as a last resource, to bring on premature labour, six weeks before the full time, and the patient was delivered of a living child, and ultimately recovered.”*

This is the only case I happen to have met with, and there are but few allusions to the operation under such circumstances in English authors. In a report of the Berlin Lying-in Hospital,† I find a reference made to its performance in cases of rheumatism of the uterus.

It sometimes happens, that the serous effusion which is usually confined to the lower extremities of pregnant females, is extended to the cavities of the pleura and peritoneum, and as it thus gives rise to a train of severe and perhaps dangerous symptoms; might not the induction of premature labour be advisable in some cases ?

On this part of the question, I confess it appears to me almost impossible to lay down definite and general rules; the decision must rest with the judgment of the medical attendant on each individual case.

7th. The only objection made by Baudelocque to his condemnation of artificial premature labour is in those cases of great ute-

* A case of fatal vomiting, during pregnancy, is related by Dr. Johnson in the *Lancet*, March 3, 1838, p. 825. “ A lady, 30 years of age, soon after marriage ceased to menstruate, and became affected with morning sickness, which symptoms were naturally enough attributed to pregnancy. The sickness, however, gradually became worse, and at last nothing of any kind could be retained on the stomach. Pregnancy was not detected, but the disorder attributed to some disease of the pylorus. The sickness and extreme emaciation were the only symptoms present. After death no morbid appearances were observable in any part of the body. The uterus contained a foetus about four months old. This patient was literally starved to death.” “ The treatment pursued consisted in the use of various salines, anti-emetics, counter-irritation, leeches, acetate of morphia sprinkled over a blistered surface, &c.”

Surely the induction of premature labour in this case, would have been justifiable, as affording the mother an additional chance for recovery.

† *Lancet* for Jan. 27, 1838.

rine hæmorrhage, before the completion of the term of utero-gestation, when the child is probably destroyed, and the safety of the mother compromised.*

Such are, most frequently, cases of “placenta prævia,” and if labour could be induced, it would afford, he says, the surest safeguard for the life of the mother. A serious obstacle to the benefit anticipated, is the length of time which may elapse between the attempt to bring on labour, and the setting in of pain. The relief sought for in hæmorrhage should be speedy, as the danger is pressing.

These are all the circumstances which have ever been considered to justify our interference in the manner proposed. I shall now proceed to detail the *method of operating*.

Five different methods of exciting uterine contractions have been adopted and recommended by different practitioners.

1. Abdominal frictions, and manipulation, with warm baths, &c. have been advised, but they very rarely succeed, their supposed advantage being the absence of unnecessary irritation.

2. Separating the membranes for two or three inches around the os uteri, will frequently bring on labour, and as this is the closest imitation of natural labour, it has been preferred by many, (*Hamilton, Riecke*.) Dr. Hamilton† remarks, “that he is now convinced, from the experience of the last ten years, that if there be a sufficient portion of the decidua separated from the cervix uteri, there is no occasion for the introduction of the open male catheter,” i. e. for puncturing the membranes. Dr. Conquest considers it as effectual as the other methods, and much safer for the infant, as saving it from pressure during the pains. If it fail, we can still have recourse to the third plan.

3. The membranes may be ruptured, either directly or obliquely, (*Conquest*.) For this purpose a female catheter may

* L'Art des Accouchemens, vol. ii. p. 288.

† Practical Observations, part ii. p. 180.

be used, or a piece of wire, or a canula, having concealed within it a spring trocar, (*Waller*.)^{*} Care must be taken to wound neither the mother nor child. From its great certainty, this method has been most generally adopted.

4. M. Klugè has proposed and practised, with great success, the dilatation of the os uteri, by means of a piece of sponge placed within it, and maintained there by a plug in the vagina. Velpeau's experience of the value of these different plans is thus expressed: "The two latter methods alone are successful. By the third, the effect is not always produced; it required three operations in the case related by M. Riecke. The separation of the membranes (the second method) is not sufficient to bring on uterine contractions; as the distention of the cervix is not permanent, the first attempt is rarely successful. Distention, by means of a piece of sponge, as proposed by M. Klugè, is much more certain. The irritation which results is permanent, progressive, regular, and sustained by the plug which is maintained in the vagina. Under the influence of such an excitement, uterine action is soon brought on, and it rarely fails to acquire sufficient energy."[†]

5. Ergot of rye is now pretty generally supposed to have the power of causing uterine contraction, and if this be the case, it will be found to be probably the most effectual and safe mode of inducing premature labour, because we can preserve to the child the safeguard of the liquor amnii, which, as we have seen, is of the greatest importance.

Dr. F. H. Ramsbotham[‡] has mentioned several cases in which it was tried for this purpose. Labour was brought on by its use alone, at the seventh or eighth month, without interfering with the membranes of the os uteri. In the Doctor's own words, "*egomet ipse tamen permulta vidi exempla, in quibus*

^{*} See Denman's Introduction, p. 322, *note*.

[†] *Traite Complet de l'Art des Accouchmens*, p. 440, Ed. Bruxelles.

[‡] *Lectures in Med. Gazette* for 1834, vol. xiv., p. 85.

partus prematurus inductus fuit, septimo vel octavo graviditatis mense peracto,—solo secalis cornuti usu ; ovuli membranis integris servatis ; ore uteri occluso neque digito, neque ullo alio modo ad patefactionem excitato.” Against this method, there has been urged the danger of the child, and it would appear not altogether without foundation. “It has happened to me,” says Dr. Ramsbotham, “in four different instances, to witness the death of the foetus, a few hours after death, by convulsions, *postquam partus prematurus inductus fuerat ope solum secalis cornuti.*”

It has been suggested, that the application of the extract of belladonna might aid in the dilatation ; but independent of the fact being doubtful, the practice would be dangerous, in consequence of the active absorption, and the development of the poisonous effects of the medicine. Dr. Montgomery has mentioned to me some such cases, which came under his own observation, and others are on record.

An interval, varying from twenty-four to ninety-six hours, (Conquest,) generally elapses after the operation, before uterine action commences, which it does sometimes, by shivering and feverishness. “Great disturbance in the nervous system,” says Dr. Gooch,* “is produced by it ; severe rigors, rapid pulse, and delirium are the occasional consequences ; but these symptoms, proceeding from nervous irritation, do not continue long enough to produce any serious consequences.”

In other cases, these symptoms were altogether absent. The patient will require the same management as after ordinary labour. It will be advisable to have a nurse in readiness to supply the infant with its natural nourishment, until the mother shall have milk for it.

After the details I have given, I venture to offer the following conclusions.

1. The induction of premature labour is not in itself immoral, and so far from being unsafe or destitute of utility,

* Lectures by Skinner, p. 223.

it does not add materially to the danger of the mother, and in many cases may save the life of the child.

2. That if the object be to save the life of the child, the cases to which it is applicable, are those in which such a narrowing of the pelvis exists, as will prohibit the passage of a full grown foetus, but not that of a younger, but still "*viable*" one.

3. That with this view, the attempt will be useless, if the sacro-pubic diameter be less than two and a-half inches, and superfluous, if that diameter be more than three and one-fourth inches.

4. That the operation should not be attempted before the completion of the seventh month, as that is the earliest period at which the child becomes "*viable*," but that it is well to allow an additional fortnight to rectify any error in the calculation of the period of pregnancy.

5. That "when this operation is had recourse to, and the dimensions of the pelvis are such as to promise success, we ought to defer the attempt as near to that period fixed by nature for the full evolution of the foetus as circumstances will admit, that thereby the child may acquire every possible advantage to insure a healthy state of existence after birth." (*James Barlow*.)*

6. That the practice should never be adopted till experience has decidedly proved, that the mother is incapable of bearing a full grown foetus alive." (*Merriman*.)†

7. That as a diminution of the lateral diameter of the lower outlets or impediments arising from immoveable morbid growths, may prove insurmountable obstacles to the delivery of a full grown foetus, either may be an adequate reason for the induction of premature labour.

* Med. and Phys. Jour. vol. v. p. 47. Baudelocque, vol. ii. p. 288.

"I have lying before me, a list of premature births, in which the periods of utero-gestation is distinctly marked. The list amounts to thirty-six cases, of eight months' children, and thirty-four of seven months. Of the thirty-six eight months' children, there died during the month of childbed, only *eight*, of the thirty-four seven months' children, there died during the month, *twenty-one*."—*Merriman's Synopsis of Difficult Parturition*, p. 182, note.

† Med. and Chir. Trans. vol. iii. p. 142.

8. In cases of extreme distortion, prohibiting the delivery of a "*viable*" foetus entire by any means we possess, a due regard for the safety of the mother, which would be more or less compromised by the severe operations necessary at the full term of utero-gestation, may justify the adoption of this operation in the early months of pregnancy.

9. The death of the foetus in utero at the commencement of the last month of pregnancy, certain disorders of pregnancy and dangerous uterine hæmorrhage, have been deemed a sufficient justification of this practice.

10. The operation should neither be resolved upon, nor performed without a deliberate consultation with other members of the profession.

11. After the operation "the utmost care should be taken to guard against the attack of shivering and fever," which is an occasional consequence.

A nurse should always be in readiness to afford nourishment to the child, until the maternal secretion of milk will take place.

Dr. S. Merriman introduces a rule which I have taken the liberty of omitting; it is this: "If upon examination before the operation is performed, it should be discovered, that the presentation is preternatural, it might be advisable to defer it a few days, as it is possible that a spontaneous alternation of the child's position may take place, particularly if the presentation be of the upper extremity.

Without questioning the possibility of such a spontaneous rectification, I must say, that I cannot but agree with the objection contained in the following quotations from Dewees and Hamilton: "Even at the full term," the former remarks, "we believe that no accoucheur would always pronounce positively on the part which may present itself to the os uteri. If, then, he cannot at the full time, when it must certainly be less difficult, and less hazardous, how can he, without a prodigious risk of being mistaken, decide at seven months, when the neck of the uterus is not effaced; when it requires some force to pass the

finger ; when it must be passed with the greatest care and delicacy, that the membranes be not ruptured ; and where, did we employ a pressure sufficient to determine the nature of the presenting part, the membranes would almost certainly give way ; we ask, under all these disadvantages, how can we ascertain, with such precision as would render the examination available, the situation of the child, at the period of utero-gestation.”*

Dr. Hamilton observes, “ even admitting that, in the ordinary course of practice, there should occur seventeen cases of præternatural presentations in seventy-eight cases of premature labour, (according to Dr. Merriman’s statement,) that would, in the author’s opinion, be an argument in favour of the induction of premature labour, for nobody can doubt that it is easier to turn an infant weighing six pounds, than one whose weight is seven pounds.”†

Dr. Hamilton further adds, that Dr. Merriman seems to suppose, that if an upper extremity present, the position of the infant may be spontaneously altered in the course of a few days ; but this is quite at variance with the author’s experience.”

ART. V.—*On Chronic Cystitis, with Observations on the Employment of the Solid Nitrate of Silver in Catarrh of the Bladder, as practised by Professor Lallemand of Montpellier.* By JOHN O'BRYEN, M. D., M. R. C. S.

[Read before the Bristol Medical Library Society.]

IT affords me no little pleasure, that I have it in my power to bring forward a subject of practical interest before this Society ; one too as yet wholly unknown in England, at least publicly ; and if by communicating it to you, the sufferings of but one fellow-citizen should be relieved or arrested, I shall consider

* *Compendious System of Midwifery*, p. 609.

† *Practical Observations*, Part II., p. 128.

myself fully repaid for any trouble I may have had in collecting the facts I shall now detail.

A residence for many months at Montpellier, and the friendship of Professor Lallemand, enabled me to observe a great number of persons affected with chronic catarrh of the bladder, and M. Lallemand's peculiar mode of treating the inveterate cases, as crowds of patients having affections of the genito-urinary organs flocked thither, even from Paris and Holland; indeed so great is his reputation, that while I was there, he was sent for to Madrid, to attend General Mina.

With your permission, I proceed first to enumerate the most prominent local symptoms in confirmed catarrh, and then cursorily to describe the state of the mucous membrane after death, and exemplify it by morbid specimens.

Chronic cystitis is generally a secondary disease, from stricture, calculus, enlarged prostate, abuse of spirituous liquors, &c., and various other exciting causes, giving rise to acute, and often to chronic inflammation in the first instance; and patients, affected with the latter, from whatever cause arising, usually complain of a dull, deep-seated pain in the hypogastric region, behind the pubes, in the perineum, and even at the end of the urethra, with a sense of weight in the rectum.

The expulsion of the urine, but more particularly that of the last few ounces, is more or less painful, according to the violence of the contractions of the bladder and abdominal muscles to expel the foreign matters united with it. When the disease is severe, and of long standing, the efforts to empty the bladder become involuntary and irresistible, causing the most excruciating torture.

The urine is generally thick, bloody, purulent, and contains much mucus, (which adheres to the *pot de chambre*,) often amounting to one-half, and even two-thirds of the whole fluid excreted.

It becomes decomposed with great rapidity, giving out a

foetid ammoniacal odour, not unlike that emitted from water containing putrid flesh. The same cause that produces this, also gives rise to an increased secretion of the magnesian phosphates.

This mucus has such great tenacity, that when poured from one vessel into another, a rope of it is formed, of some feet in length, in fact, when unmixed with blood or pus, it nearly resembles the white of egg in appearance. It is always alkaline, even though the urine may sometimes be acid, as observed by Dr. Prout; it affords a nidus for the deposition of phosphates, and occasions what we shall call *mortar*.

Remark this, gentlemen, the urine may be acid, and if you relied on this alone, you might be tempted to order alkalies, as lime water, &c. &c.; a very serious mistake for the patient, as by this means, you increase the deposit of the phosphates, and ammonia being formed by the decomposition of the urine, you may in this way give rise to calculi of the triple phosphates, or at least cause a great aggravation of the sufferings of your patient, which might so affect the general health, as *indirectly to occasion death*; and beside mucus, blood, and pus, &c. &c., the urine contains more or less prostatic and spermatic liquor. The calls to make water became very frequent, each time accompanied by pain more or less severe, and in some cases the urine escapes as fast as it *is excreted by the kidneys*. But permit me here to transcribe a sketch of some of the symptoms of this disease, as related by Cruvelhier to have occurred in a patient of his, a drawing of whose bladder may be seen in his Pathological Anatomy, and to which I beg to refer the reader, as it will do more to give a correct idea of the sufferings of patients from this affection, than any description I could possibly give, particularly as to the violence of the involuntary contractions of the bladder, and the pain produced by them. This patient, after an illness of some time, presented the following symptoms, in the words of M. Cruvelhier. “Alors se manifestèrent des besoins fréquents et impérieuse d’uriner avec contrac-

tions douloureuses de la vessie et douleur à l'extrémité de la verge, les urines devinrent catarrhales, le malade presenta tous les symptomes du catarrh vesical.

“ An bout de quelque temps, les douleurs devinrent tellement vives et tellement rapprochees qu'il lui fut impossible de quitter la chambre.

“ Le malade survécut trois mois d'agonie, les *besoins d' uriner, frequents, pressants, douloureuse contractions violentes de toutes les puissances expulsatrices*, tellement que le *defacation* accompagnait souvent l'émission des urines.”

After death the morbid appearances are various, but generally in proportion to the local symptoms observed during life. The fundus of the bladder is usually most diseased, but the whole of it is thickened, contracted, and harder than natural, and its mucous membrane is purplish, injected, and presents many folds or columns of greater or less thickness, according to the period of the disease and their situation. They are always longer and larger at the fundus. This thickening of the walls, as well as the columns which the lining membrane presents, in consequence of which the French call it *la vessie a columnes*, as they are not unlike the *columnæ carneæ cordis*, are both exclusively owing to the hypertrophy of the muscular coat, some fibres of which have become more developed from increased action.

The most prominent portions of these folds are usually of a blue or purplish red colour; while between them, the membrane is pale, though still, like the rest of it, swollen, soft, and offering little resistance; occasionally small ulcerations are found. But what is very remarkable, between the folds, pouches or sacs generally coexist with dilated ureters, both being produced by the same physical cause, (forced distention,) in consequence of prolonged difficulty in expelling the urine; the formation of the former may be explained in the following manner: the contraction of the abdominal muscles, as well as of the bladder itself, when full of urine, forces portions of its

lining membrane between its muscular fibres, and thus sacs or pouches are formed, and these pouches being lined by a diseased mucous membrane, secreting an alkaline mucus, sometimes become the receptacles of the mortar-like matter, and finally, of calculi, generally consisting of phosphate of lime.

This latter point is one of great practical importance in the pathological history of the bladder, and which, I believe, has not been noticed by any writer except Sir B. Brodie in his lectures, and even he mentions nothing concerning the calculi formed in the pouches as differing from encysted calculi. Andral, I am aware, mentions the *fact* that stones were found in the sacs without explaining how they came there. To exemplify this, and prove it as every point in practical medicine should be *proved*, I mean by a reference to symptomatology and morbid anatomy, *taken connectedly*, I shall, with your permission, read a case, and bring forward for your inspection a morbid specimen, for which I am indebted to Mr. Eslin's kindness; the case has been published (in the Medical Gazette for December, 1830) since the day on which this paper was first intended to be read: you will, for its great value with reference to my subject, excuse my giving an abstract of it, and accompanying it by a few remarks. But permit me first to remind you, that when from indigestion, or from a quantity of acid fruits eaten, or from any of the mineral acids taken into the stomach, the urine contains a free acid in any quantity more than normal, uric acid is precipitated even in the bladder, (in consequence of its feeble affinities,) and thus a *nucleus may be formed*; and when from having taken alkalis in abundance, and more particularly at that period of life at which the constitution begins to break up, when the urine becomes naturally alkaline without any acid from without; if, I say, at this period of life chronic cystitis exists with its alkaline mucus, and the urine also become alkaline, then (and this is highly practical) if there exists an obstacle to the free egress of the urine, and much more so if its expul-

sion is impossible except through instruments ; then, I repeat, mortar will most probably be formed, and also be lodged in the pouches of the membrane, and finally become calculi. This theory, if you will call it so, is not speculative, as it points to the indications of relief, if not of cure. The mineral acids should be taken internally, and the bladder be daily washed out with a weak solution of nitric acid, forced from a gum-elastic bottle through a catheter a double current into that viscus, will, I take it, fulfil three indications ; first, to relieve the symptoms of chronic cystitis ; secondly, to render the urine acid, and thus in a great measure prevent the deposit of the phosphates ; and thirdly, to wash out the bladder, and prevent the stagnant mucus accumulating in the pouches.

I shall now leave it for you, gentlemen, to judge whether this is not what took place in the case of the patient, an abstract of whose history I am about to read.

This case, as published in the *Medical Gazette* for December 30th, 1837, is thus headed :

“ Retention of Urine from Tumours within the Bladder, with Sacculated Bladder, and a large Accumulation of Calculi. BY MR. ESLIN, Bristol.

“ Before his illness, Mr. Jones, the subject of this communication, had occasionally experienced some difficulty in passing his water. The urethra was examined for stricture, as well as the state of the prostate gland, but no adequate cause for any obstruction could be detected. On his recovery, November, 1827, he was unable to make water ; there was no obstacle to the introduction of a catheter, nor was any calculus discoverable ; he was taught to use the catheter himself, and he introduced it from four to six times in the twenty-four hours. At first he used a silver instrument, but he afterwards gave the preference to the gum-elastic catheter.

“ For the last twelve months his health had more declined, and he was compelled to draw off his water more frequently

which at times gave him great pain, and when this was the case, it was scanty, and *contained much purulent mucus*. Sometimes small scales of calculous matter stuck in the apertures at the point of the catheter, and rendered its withdrawal painful, and whenever, some months before his death, a silver instrument was used, it was felt to grate against a calculus.

“ On the 7th of February, 1837, he was seized with influenza, and he became much exhausted by it; the disorder of the bladder was more troublesome, the urine requiring to be drawn off every hour, and he died eleven days from the invasion of the influenza, in *the sixty-sixth* year of his age.

“ *Post Mortem Examination*.—On opening the abdomen, the fundus of the bladder was seen above the pubes, apparently distended with urine, but really containing very little; its coats were thickened, and its surface irregular, several rounded prominences being observed upon it. These projections, upon being touched, were found to contain calculi; during the unavoidable moving of the bladder in the progress of dissection, the calculi fell out of their cysts, and were heard to rattle within the bladder.

“ *Appearances of the removed Parts*.—The right ureter was much thickened in its coats, and enlarged in its calibre; the little finger could be introduced into it; at its connexions with the bladder, at the point where this canal entered, it felt almost cartilaginous. The left ureter was a little larger than natural. The bladder and prostate gland were slit open from the caput gallinaginis, to the fundus. The prostate gland was increased to the size of a small orange; attached to the prostate within the bladder were three distinct tuberculated, semi-cartilaginous excrescences, two of them were on the left side of the urethra; one was very prominent, and loosely adherent by a narrow neck, and was the size of a small hazel-nut. The next, more in contact with the urethra, was less circular, and appeared more like an irregular projection of the gland itself. The third, on the right side, was the largest tubercle, and was firmly attached to

the prostate. When the divided portions of the prostate were put in apposition, it was clear that these tumours entirely closed the aperture into the urethra from the bladder, the looser one might be aptly compared to the bullet valve of the stomach pump. Within the bladder an extraordinary number of calculi presented themselves of all sizes, from that of a moderate Spanish nut, to the dimensions of a small sweet pea seed; many of these were still embedded in pouches formed between the muscular fibres of the bladder, and similar cavities, from which other stones escaped, were very numerous.

“Of these stones that were removed, there were thirteen weighing, when dry, from four drachms and a half to ten grains each, and about fifty smaller ones, weighing together two drachms and a half; their total weight was rather less than four ounces: some of the smaller stones could not be removed, (as they were so imbedded in their pouches,) without squeezing them out, in consequence of their being larger than the mouth of the cavity which contained them, a proof that they were *formed where they were found*. In those parts of the bladder where they were found, the columns or folds (an unvarying appearance in chronic cystitis) were so strongly marked, as to give the surface very much the appearance of the columns found within the ventricles of the heart.

“A peculiarity in this case was, that nearly all the calculi were tetrahedral, even the minutest exhibiting the same appearance; very few shewed any marks of attrition, and several were so situated as to be prevented from rubbing against others, by their firm enclosure in their respective sacs. In the beautiful collection of calculi in Mr. Richard Smyth's museum at the Bristol Infirmary, consisting of 400 specimens, Mr. Eslin could not find one to resemble those in his interesting case.

“At what period these concretions began to be formed it is impossible to determine; had any existed at the time when the inability to make water became confirmed, it is not probable that they would have escaped detection during the various explo-

rations of the bladder by the metallic catheter then used. For several years he used elastic gum instruments, and had rarely occasion for the assistance of a surgeon.

“ The calculi, according to the analysis of a friend, consist almost entirely of phosphate of lime ; a white sand, which coats most of the specimens, and is found to intervene between some of the layers of the solid parts, is uric acid.

“ More irritation existed for the last year or two, purulent mucus was oftener drawn off, and a more frequent introduction of the catheter was required. As the bladder did not contract, the ordinary sufferings of this distressing complaint were not experienced.”

When we pass in review the symptomatology of this case with reference to the morbid appearances found after death, it appears abundantly evident, that before 1827, Mr. Jones's difficulty in passing water was attributable to what is not uncommon amongst old men, an enlarged prostate, and it is also probable, that after his recovery in November of the same year, the excrescences found attached to that gland, caused the impossibility that then manifested itself, of passing water unaided by instruments ; now this difficulty in emptying the bladder from enlarged prostate acted in the same way as a stricture of the membranous portions of the urethra or paralysis, which, by causing retention of the urine in that viscus, is always a very active exciting cause of that distressing complaint, chronic cystitis.

That this disease existed a year or two before his death, there does not appear a shadow of doubt, “ as the water was obliged to be drawn off more frequently, and it contained *much purulent mucus*, and the act of drawing it off was accompanied by much pain.”

Now as the disease existed so long, it necessarily follows that that constant morbid appearance before described, I allude to a sacculated state of the bladder, also existed.

You will recollect what I said when treating of a peculiar

termination of chronic catarrh under given circumstances. Let us see if in Mr. Jones's case these existed.

1st. He was sixty-six years of age, the period about which the urine naturally becomes alkaline.

2ndly. There existed a permanent obstacle to the natural expulsion of urine, (enlarged prostate, &c. &c.) The case presents an interesting example of a bladder, that for nine years had never evacuated its contents by its own efforts.

3rdly. Chronic catarrh, with its alkaline mucus, and a sacculated bladder ; and

4thly. After death, these sacs were found full of calculi, consisting of phosphate of lime.

From these data, we may conclude, 1st, that the chronic catarrh was caused by the enlarged prostate. 2ndly, that the catarrh gave rise to the sacculated state of the bladder, and filled the sacs with alkaline mucus, which, as the viscus could not expel its contents, remained undisturbed in them ; the age of the patient, and the advanced stage of the disease, make it all but positive that the urine was alkaline, and if I may be permitted to judge from effects, the formation of the phosphate of lime calculi prove it to have been so, and thus the calculi were formed. *Query*, would not the treatment I have proposed have tended to relieve the catarrh, and to have prevented the accumulation and stagnation of the mucus, also the deposition of the phosphates, and thus fulfil the indications for which it was proposed. As to the tumours found within the bladder, let us see what the father of morbid anatomy says about excrescences attached to the prostate.

Morgagni has established, that a vegetation, often having a pedicle, (and in this case we have good specimens of it,) is frequently met with in old persons ; it usually arises on a level with the orifice of the urethra in the bladder, from the inferior wall of the prostatic portion of that canal, near the verumontanum, and this is often prolonged into that reservoir, and acts as a valve, effectually preventing the egress of the

urine, and (if the lateral parts of the prostate are normal) giving rise to a well-marked symptom. The stream of urine, at first large, is suddenly stopped, and differing in this from stone, by the effect of change of position in the latter. This tumour is not of the nature of polypi, but is glandular, and composed of the same substance as the prostate. This view has been confirmed by Valsalva, Fantoni, and Desault, and in this country, by Hunter and Sir Everard Home, who called the centre eminence, if there are three, the middle lobe ; but M. Cruvelhier denies the existence of sufficiently defined or separate lobes. Complete retention of urine was the consequence of this disease in Mr. Eslin's case, as well as in the one cited from the *Pathological Anatomy* of M. Cruvelhier.

I shall now resume the general description of the morbid appearances in chronic cystitis.

When the disease has lasted a long time, the membrane lining the ureters and kidneys assumes much the same appearance as that of the bladder itself. Indeed destruction even of the glandular structure of the kidney, is sometimes met with, a case of which I shall cite presently. The ureter and pelvis of each kidney are generally dilated, and this dilatation is greatest where there has been a long continued difficulty in expelling the urine from the bladder, as in the succeeding case ; but it may exist in other cases, though in a less degree, and in this specimen you will perceive this pathological state well marked.* As morbid preparations are only really valuable when contrasted with symptoms during life, I shall trouble you with a few succinct extracts from the history of the case of the patient from whom I took this bladder and kidney.

He was a single gentleman, about forty years of age, living at Montpellier, Bristol, who for many years suffered from an affection of the stomach, the leading symptoms of which were, vomiting in a minute or two after the simplest article of diet was

* A well marked specimen of this disease was here exhibited.

swallowed, a fixed pain and tenderness on pressure in the epigastrium, and occasional hæmatemesis ; and this patient, in order to relieve his continued sufferings, commenced, about a year and a half before his death, to drink, at all hours of the day, large quantities of ardent spirits, which habit he continued to the close of his life ; a year before which, and six months after he began to drink spirits, he observed that he made water more frequently than usual, but in smaller quantity, and with some difficulty and pain ; this he attributed to an old stricture.

The urine deposited a thick, transparent, greyish matter, like starch, which adhered to the *pot de chambre*. This gradually increased in quantity, and its colour became dark, like coffee grounds. It had a putrid smell, and its expulsion often occasioned a torturing pain, referred to the perineum and glans penis, and for the last two months of his life, the calls to make water, which was acid, became so frequent and painful, that he never enjoyed one hour's continued rest, notwithstanding the quantity of alcohol he drank. He died exhausted and emaciated, more from the affection of the bladder than that of the stomach, as for three months the fixed pain and tenderness on pressure was not perceived, and vomiting was only excited by some indigestible food, or too large a quantity of any fluid taken at a time.

Post Mortem Examination, twenty-four Hours after Death.—In the small curvature of the stomach, about its centre, was found a perforation, caused by an ulceration, the edges of which were regular and healed ; the contents of this viscus were prevented from escaping into the peritoneum by the adhesion of the transverse colon, the peritoneal coats of which, covered with effusion of lymph, formed the bottom of the perforation. All redness had disappeared, and thickening alone remained to shew where inflammation had existed. This stomach, with one kidney, I sent to the Medical School, Bristol. The ureters and the pelvis of each kidney were enor-

mously dilated, yet their lining membrane remained healthy; the glandular structure, you may perceive, has been in great part absorbed, in consequence of pressure from the distended pelvis, and the exterior of it has a lobulated appearance. This is the left kidney and the bladder; the latter is thickened and contracted, and its membrane presents a good specimen of the folds above described, as well as one or two of the pouches; its dark-red colour has disappeared, but this drawing in Mons. Cruvelhier's work will convey a correct idea of what it was before it was put into spirits. About three inches from the glans penis I found a circular stricture, caused by thickening of the sub-mucous cellular tissue, through which a large director could be passed, and behind it the membranous portion of the urethra was much dilated.

In this case there appeared two well marked exciting causes of chronic cystitis: 1st, an old stricture, which it is more than probable had been increased by spasm; 2nd, the abuse of ardent spirits.

I could make many remarks on this case, interesting as well on account of the diseased stomach, as the bladder, but my subject obliges me to proceed. From what precedes it appears to me easy to conclude, that the local symptoms are quite sufficient to establish the diagnosis of chronic cystitis, without any reference to the general ones, which are often uncertain, and may arise from other diseases. After the diagnosis, nothing is of greater importance than a correct knowledge of the exciting causes, both of the affection itself, as well as of its complications; and to arrive at this information, it is not sufficient to know the state of the urine, the constitution of the patient, or even the circumstances which caused the disease, but the urethra, the prostate, and the bladder must be examined with great care, and the disease treated accordingly. And in order to show you that Mons. Lallemand does not trust exclusively to the method to be spoken of presently, I shall simply enumerate the various modes of treatment

employed by him, from notes taken at his clinical lectures, and from numerous observations collected by myself at the Hospital of Montpellier.

If he suspects the system to be tainted with syphilis, he orders either cyanides, iodides, or the bichloride of mercury; and he relies much on the preparations of gold in secondary syphilis. If the functions of the skin are out of order, (and too little attention is paid to this source of disease in this country,) sulphur and its preparations are used both internally and in baths, narcotic lavements, local bleeding, blisters, mucilaginous drinks, camphor, opium, resins containing turpentine, the balsams of tolu and copaivi, and tar water. Besides these, he uses injections of different descriptions, in the ordinary way, and with the catheter "*a double courant*" into the bladder. But there is one mode of treatment, namely, the use of saline purgatives, which I regret is so much neglected by the French, and which he does not adopt. It is known to be of great service in relaxing urethral spasm, and I doubt not would have saved much suffering to several of his patients, especially if followed by a large dose of opium.

When all these means have failed—what is to be done?

All authors, I believe, and Sir Benjamin Brodie amongst them, agree in asserting, that chronic cystitis, when arrived at this stage, is incurable; and I would ask, what general treatment could be expected to remove such organic changes in the mucous membrane, as the *post mortem* appearances prove to exist in every case presenting the symptoms of chronic catarrh?

Sir B. Brodie, when speaking of this disease in his lectures, says, nitric acid injections may be used with advantage; but he continues, "there is no disease for which an improved method of treatment is more wanted than for this, which has hitherto been the opprobrium of surgery." Analogy and chance taught M. L. that it was not incurable, but on the contrary,

a disease over which medicine possessed a great control. For many years he was in the habit of using solid nitrate of silver, with great success, in the treatment of gleet, ulceration of the urethra, vagina, and os uteri, as well as in the purulent ophthalmia of children; we know the effect of lunar caustic in chronic ophthalmia, and all these are diseases of mucous membranes similar to that of the bladder, and subject to the same mode of treatment. I say chance taught him, for when applying caustic to the neck of the bladder, the instrument slipped and cauterized the lining membrane of that reservoir; and to his great astonishment, the patient was cured of a chronic catarrh, as well as the affection for which it was used intentionally. Speaking of this, in one of his lectures, he used the following language: “*La premiere fois que cette accident m’arriva, J’eus pendant plusieurs jours des vives inquietudes, sur les consequences qui pouvaient en resulter; mais il ne se manifesta aucune phenomene facheux.*”

Now, gentlemen, if you reflect upon the severity of chronic cystitis, which irresistibly compels a man to get out of bed four or five times per hour during the night, each time subjecting him to excruciating pain, and thus effectually preventing sleep, and which will not permit him to sit one half hour at his dinner table without disturbing him; to say nothing of the effect of pain on the general health and the constitution, or the want of exercise on horseback or in a carriage, or even on foot; and then again reflect, that when of long standing it has been declared incurable, inveterate, and the opprobrium of surgery, though it may be relieved, but like chronic bronchitis, returns with redoubled severity upon the application of the slightest exciting cause; I feel assured you will agree with me, that any mode of treatment, offering for such a disease, not relief alone, but a radical cure, and that too, not in months, but weeks, and often in a few days, deserves our best attention, and its author our meed of praise. M. L.’s work, too, on the diseases

of the urethra and prostate ; his late simple, safe, yet beautiful mode of curing nævus, and his letters upon the brain, point out the kind of results we might expect from such a man ; in fact they have procured for him an European reputation as a correct practical observer. Need I say any thing to those who have read his articles in the various dictionaries, except to repeat his name, in order to insure their attention.

Professor Lallemand, then, has successfully practised cauterization, with solid nitrate of silver, of the mucous membrane of the bladder in chronic catarrh.

You will, perhaps, ask how lunar caustic acts upon the membrane, and what are the modifications it causes ? This I shall endeavour to prove by analogy, first reminding you, that this caustic has been employed, and that successfully, in the cure of even croup, by Dr. P. de Bisson of Paris. I suppose his cases were spasmodic, or if inflammatory, the disease to have commenced in the pharynx ; the author proposed to introduce the caustic into the larynx. I believe the effect of lunar caustic on diseased mucous surfaces has not been hitherto *sufficiently appreciated*.

You have all seen patients affected with chronic inflammation of the conjunctiva, often of many years' standing, against which a great variety of treatment had been employed *unsuccessfully*.

I now allude to an entirely local disease. Apply nitrate of silver to the injected, swollen, softened, and ulcerated membrane of the eye ; immediately the patient feels a hot, burning pain, the secretion of the lachrymal gland increases, dissolves the caustic, and distributes it equally over the membrane ; at the end of an hour, if the application was slight, the pain diminishes, and light is more supportable ; in a day or two the membrane has a different appearance ; the chronic has become an acute inflammation, which has a peculiar tendency to end in resolution, and the surface of the eye is of a vivid red ; by degrees this inflammation increases, and the

membrane becomes less red and swollen. If in a few days resolution has not taken place, caustic is re-applied, and the same thing again occurs; the membrane becomes more and more natural, its vessels get more tone, and expel their contents, it looks more white and shining, in fact, it has returned to its normal condition. What has been observed in the conjunctiva, may explain what occurs within the bladder. Monsieur Lallemand uses an instrument of the following description, indeed he himself chose this one for me. It consists of a large catheter (of pure silver, as, if there is any alloy, the caustic acts upon it) open at both ends, having two sorts of stilet, according to the part you wish to cauterize; at the extremity of each stilet is a small excavation, containing the caustic, which is first pulverized, and then placed in the excavation over a spirit lamp, which fuses and moulds it to the cavity.

When the instrument is prepared, introduce into the bladder an ordinary catheter, in order to empty it completely. This *precaution is strictly necessary*, for the urine would dissolve the caustic, and prevent its directly affecting the mucous membrane: when this has been withdrawn, the instrument bearing the caustic is to be introduced, (closed,) and the moment it has entered the bladder, you are to push the stilet, and rapidly turn the *porte caustique* from side to side two or three times, and then pull the stilet into the instrument, and withdraw it; our object should be to touch the surface in as many points as possible. While the instrument is within, the bladder contracts and grasps it, the kidneys secrete a small quantity of urine, as the lachrymal gland secretes tears when the conjunctiva is cauterized; but this small quantity of liquid, far from being hurtful, is on the contrary favourable, as it acts as a vehicle to the portion it does not decompose, and conveys it equally over the surface of the membrane. The patients feel, at that moment, a sharp pain at the neck of the bladder and in the rectum, described by them as not unlike a pinch, but much more supportable than the continued dull pain of chronic catarrh; there is now an irresistible desire to pass wa-

ter, and as the bladder is nearly empty, very little passes, and this causes a burning along the urethra, and is accompanied by some drops of blood. This desire is renewed every moment, causing violent but useless efforts. These gradually decrease, and on the second and third day there is no longer any pain on making water, and a few small grey eschars, like burned paper, come away with the urine. This occurs in a large number of patients, but in some more susceptible, the process does not proceed so simply, particularly if you have used the *porte caustique* too long. In this case retention of urine follows, which lasts from three to thirty-six hours; even here we must not be in too great a hurry to use the catheter, as a warm bath, a few narcotic lavements, emollient drinks, some tartrate of soda, with infus. sennæ, and sometimes a few leeches, will cause the spasms to yield; if not, some belladonna to the meatus may be tried, always taking care to use antiphlogistics with moderation in the beginning, as inflammation is necessary to the cure. In a majority of cases one cauterization is sufficient to procure a cure; when it happens otherwise, a second and even a third application may be necessary, but Monsieur L. states that he never saw a case requiring a fourth.

Gentlemen, always bear in mind that this treatment cannot succeed when the kidneys or prostate are diseased. Compare this with the modes of treatment hitherto employed; with what perseverance must they not be continued in for months, nay often for life, to procure even relief.

Patients treated as formerly, must use great precaution to prevent a relapse; the least fault in diet, the slightest cold, or change in the temperature of the air, the most moderate exercise on horseback, or in a carriage, may act as a match fires a train, and make the sufferer as bad as ever; in fact, they must be always on their guard, even to enjoy a supportable state of health.

This is not the case with cauterization, for, however feeble the digestive organs are, however weak the patient,

whether in health or constitution, it may be employed with advantage, as it is never injurious; it acts directly on the diseased surface, and does not affect the other viscera; and the patients, instead of being months and years under treatment to procure even a slight amelioration, may be cured in a few days, or at latest, in a few weeks. The cure is not an uncertain one, and the patient may return to his ordinary mode of life without fear. These results are sufficient to render the use of this means general, nay to make it preferable to those employed every day, as it may be modified in slight cases, by using an injection, containing the same salt in a weak solution. But, gentlemen, this is not all; IT ALONE SUCCEEDS in a large proportion of the cases which have hitherto defied the most *experienced surgeons*.

I could have wished to have been able to present, for observation, and contrast, a bladder from a patient who had been affected with well-marked symptoms of chronic cystitis, who had been treated by cauterization, and was cured, but died of another disease afterwards. Opportunity has not so favoured me; although I have seen a large number treated by this means, only one death occurred, to my knowledge. That patient, the history of whose case I have to detail, as well as the post mortem appearances, was a Dutch gentleman, who for fifteen years had three strictures, with two fistulæ, one on the dorsal side of the penis, and the other *in perineo*; he had been under treatment by Delpech, and confined to the house, and latterly to bed, from July, 1826, to March, 1837. Before the operation of applying nitrate of silver to the lining membrane of the bladder was tried, as a forlorn hope, the patient, besides suffering from aggravated symptoms of cystitis, was affected with continued hectic fever, shiverings, want of sleep, and continued gnawing pain. The abdomen was tense, and extremely painful to the touch, and the bladder formed a distinct and exquisitely sensitive tumour above the pubes. After the operation he became decidedly better, but died in three months.

In the place of one kidney, a cyst of pus was found, the other was full of abscesses, and the bladder greatly thickened, ulcerated, and actually perforated in about a dozen places. Peritonitic adhesions, and a variety of other chronic lesions, were found. Now it will not be supposed that any treatment could have succeeded in such a case. I feel persuaded, that the patient's life was prolonged, and the process of ulceration stopped for a time, by the *application of nitrate of silver*. I shall trouble you with two cases in illustration of the effect of this remedy, which I have chosen from amongst a number, collected at *Montpellier*, from the clinical lectures of Monsieur L.

CASE I.—A commercial traveller put himself under the care of Monsieur Lallemand, 1832, ætat. about 27, of a naturally robust constitution, but much deteriorated by dissipation. He stated, that up to the age of twenty-one he was much addicted to masturbation. At this period, his own account of his excesses and bestiality would scarcely be believed in this country. After having had ten successive gonorrhœas, he began to feel a deep-seated pain above the pubis, which was accompanied by frequent hæmorrhage from the bladder. The volume of the jet of urine diminished gradually, and its emission became more frequent. When he arrived at Montpellier, he was obliged to make water with the almost incredible frequency of twenty times per hour, and then only drop by drop, followed by severe pain. It deposited a quantity of purulent and adhesive mucus, constituting half of the fluid excreted, and which, when poured from one vessel into another, might be drawn into a thread of many feet in length.

He was ordered rest in the horizontal position, leeches to the perineum, baths, vegetable diet, tar water, one quart per day, and various tisans, stimulant and diuretic. This treatment was continued for forty-two days, without any benefit. In March, 1832, Monsieur L. introduced a silver catheter,

which caused violent pain when it arrived at the neck of the bladder. Baths, lavements, and repose for two days more, and on the third, Monsieur L. cauterized the mucous membrane of the bladder, in the manner before described, and as extensively as possible. Complete retention of urine, which lasted thirty hours, followed, although baths, lavements, and leeches were ordered. *Quere*, would not an active purgative of some neutral salt, and inf. sennæ, followed by a large dose of opium or belladonna, have succeeded in reducing the spasm? As the patient suffered much, he wished the catheter to be passed, but Monsieur L. refused, as he said that the spasm would soon yield, and the patient being again placed in a bath, the bladder was naturally emptied, he passed a good night, and the following day was free from pain. From this period the amelioration was progressive and rapid, the necessity for making water became less, and the sediment diminished in quantity and in viscosity; the presence of pus could scarcely be discovered, and in twenty days after the operation, this traveller made water only eight or nine times in the twenty-four hours, and when he quitted Montpellier, the urine was natural and limpid.

This observation shews, in a forcible manner, all the advantages to be derived from the application of nitrate of silver to the mucous membrane in chronic catarrh. The long duration of the disease, the repeated hæmorrhage from the bladder, the purulent state of the urine, the well marked viscosity of the mucus, its quantity, and the great frequency of the calls to make water; all shewed a disease incurable by any means hitherto employed: one *simple* cauterization was sufficient in this case to arrest all the well marked symptoms of chronic cystitis, because the prostate and kidneys were healthy.

CASE II.—Mons. R., a Polish officer, contracted his first gonorrhœa in 1831, which was treated by the balsam of copaivi, and which nearly disappeared in spite of the fatigues of the Polish campaign. In 1832, he had a second gonorrhœa,

which this time resisted all the means employed, and became chronic.

When this patient entered the Hospital of St. Eloi, he complained of a stricture at the prostatic portion of the urethra, with a gleet, and a dull pain at the fundus of the bladder. The emission of urine was difficult, and the urine itself was muddy, and deposited at the bottom of the *pot de chambre*, a greyish, purulent, and bloody sediment in so large a quantity, that it formed two-thirds of the liquid excreted. He was ordered leeches, baths, both general and local, &c. &c. After twenty-five days' continuance of this antiphlogistic treatment without any benefit, eight pills of chian turpentine, each containing four grains, were ordered for him in the twenty-four hours, to be increased to twenty per day; after three months the pain and uneasiness were a good deal diminished, but the urine continued thick and bloody, and the gleet not improved. In the month of August, four months since he entered the hospital, Monsieur R. went to take the benefit of the sulphureous waters of Bagneres de Luchon, but having contracted ague, he left the Pyrennees without having received any benefit from the waters. Fatigued and disgusted with medical treatment, having employed it so long without success, he made up his mind to content himself with a vegetable diet and a few baths, and to bear his sufferings with patience; but he became so ill, that he was obliged to enter the hospital at Montpellier again, in the month of December, when Monsieur L. cauterized the stricture and the membrane of the bladder, and ordered a bath, a lavement, and a few leeches; during the next two days, the urine continued muddy and red, the sediment being very abundant, but on the fourth, the sediment was much less, and the expulsion of the urine less painful and difficult. For the next two days there was a gradual, but progressive improvement in the state of the patient, but in about a month after the first cauterization, the disease appeared no longer to improve, but rather to get worse, and Monsieur L. again applied the nitrate of silver; retention of

urine followed, which lasted nine hours. Baths, lavements, and leeches were repeated; the patient passed in the bath some bloody urine mixed with pus: in a few days the urine became more abundant in quantity, and less muddy; and on the sixth it was clear, and contained a very little gluey matter; and on the eighth day Mons. R. quitted the hospital to go to the baths of Arles, where he remained some time.

In 1834, I saw this Polish officer at the Hospital Montpellier, where he came to be treated for a jaundice, and he declared to me, he never had the slightest return of the bladder catarrh, as he called it, since he quitted Montpellier to go to Arles. I examined his urine, which was clear and limpid, though yellow, and it did not contain the smallest trace of thick mucus, though two years had elapsed since nitrate of silver was applied to his bladder.

Now, gentlemen, place these two cases together, as they were produced by the same causes, and each patient had undergone many long and tedious modes of treatment, and all of them without success, and you must be struck with the rapidity with which one or two cauterizations removed a disease that resisted so many other means. Why was it that in the second case a second cauterization was necessary? because the disease was a much more severe one, two-thirds of the urine being sediment, and besides this, the stricture prevented Monsier Lallemand using the ordinary large *porte caustique*, and he was obliged to use a small one, in consequence of which the diseased mucous membrane was not so extensively cauterized as it *ought to have been*.

I shall now leave it for you to judge of the efficacy of the means I propose, and to draw the conclusion as to its comparative superiority, which appears so obvious to me.

ART. VI.—*On the Genus to which the Worms known by the name of Ascarides belong.* By O'BRYAN BELLINGHAM, M.D., M.R.C.S. &c., Surgeon to St. Vincent's Hospital.

THE entozoa which inhabit the intestinal canal of the human subject are, in general, objects of such every day occurrence, and their external characters and internal organization have been so often examined and described, that little appears to have been left for subsequent observation.

The symptoms by which their presence in the intestinal canal is indicated, and the appropriate treatment against each particular species, are to the medical practitioner, the most interesting points connected with them, and claim his particular attention: to the zoologist, however, they are also objects of much interest, and this is not the only point at which natural history and medicine approach each other.

My object, in the following short communication, is to notice a controverted point in the zoological characters of a particular species, the *ascaris vermicularis* of Rudolphi, the *oxyuris vermicularis* of Bremser. It belongs to the order hematoidea in Rudolphi's arrangement, and to the division "vers intestinaux cavitaires" of Cuvier's "regne animal."

The natural order hematoidea contains eleven genera, and Rudolphi has placed the intestinal worms, known by the name of ascarides, in the genus *ascaris*, while Bremser (as high an authority) has referred them to the genus *oxyuris*, and subsequent writers have classed them as they copied Rudolphi or Bremser.

The genuine characters of the order hematoidea are taken principally from the shape of the mouth, the existence or not of lips or tubercles, &c. The genus *ascaris* is characterized by three tubercles, which surround the orifice of the mouth, one of which is superior and two are inferior: these parts are much larger and more prominent in some species than in others, and

are easily seen with the naked eye in the *ascaris lumbricoides* of the human subject. The genus *oxyuris* is characterized by an orbicular mouth, without either lips or tubercles; its character may be readily seen in the largest species of the genus, the *oxyuris curvula*, which is very common in the cæcum of the horse.

Now, as the *ascarides* have been referred by Rudolphi to the genus *ascaris*, and to the genus *oxyuris* by Bremser, the characters of which are sufficiently distinct; it is evident that one of these authors must be in error, and yet, I could hardly believe, that the accurate Rudolphi, who has given us the fruits of so much laborious research, in his different works on the entozoa, could be mistaken; and I was equally disinclined to think that Bremser either could be wrong; and yet, how were their contradictory statements to be reconciled on a point which required merely accuracy of observation?

It occurred to me, (although not a very probable supposition,) that there might be two distinct species inhabiting the human intestines, one of which had been described by Rudolphi, and the other by Bremser; however, on referring to their works, I found that each had examined and described specimens coming from the same individual. Considering it to be a subject not altogether unworthy of investigation, the first time I happened to meet with these animals, I put some into spirits of wine, and set them aside for examination; some weeks elapsed before I had leisure to do so, when I found that the mouth had apparently the orbicular character of the genus *oxyuris*; not being perfectly satisfied, however, I determined to examine more recent specimens, and the next time I met with them, I put some into water, and submitted them to examination very shortly afterwards; when, without any difficulty, I could see (with a lens of about a line focus) the three small tubercles surrounding the mouth, which characterize the genus *ascaris*, the existence of which Bremser has denied.

Having placed them in spirits of wine, and submitted them

to examination a few days later, these parts were much less distinct, the mouth had apparently a circular shape, owing to the retraction of the tubercles, but within the orbicular margin they were still visible. Before I had put them into spirits of wine, the three tubercles of the mouth formed the most prominent part of the anterior surface of the head, but after having remained a certain time in the spirits, they were the least prominent, and appeared to be drawn in, having an external circular orifice, which bore a very close resemblance to the mouth of the oxyuris.

Such then appears to me a probable cause of the error into which Bremser has fallen, in removing the animal under consideration from the genus *ascaris*, and placing it with the *oxyuri*, to which it cannot now any longer be referred, its generic character being perfectly distinct.

With respect to the frequency of the occurrence of this parasite in the human intestines, although set down in many works as the most common, it is much rarer than the *trichocephalus dispar*. I examined successively the intestinal canal of fifty-five individuals, who died in St. Vincent's Hospital, and was able to detect it only in ten, and but once in any considerable number; whereas the *trichocephalus dispar* occurred in forty-nine out of the fifty-five cases, and what is remarkable, in none of these instances did I find the *ascarides* in the rectum, which is looked upon to be their natural habitation; they occupied the ileum, cæcum, and colon, principally the cæcum. Another circumstance, not unworthy of notice, connected with this species, is the rarity of the male, all the specimens which I have hitherto observed being females.

ART. VII.—*On the Primary Causes of Strangulation, and an improved Mode of performing the Taxis, in Cases of Intestinal Herniæ.* By JAMES O'BEIRNE, M. D., one of the Surgeons to the Richmond Surgical Hospital, &c., Dublin.

IT is perfectly manifest that accurate views of the causes of strangulation in external intestinal herniæ, and of the altered state of the prolapsed and other parts of the intestinal canal, in such cases, present the only certain means of ever arriving at a knowledge of the most successful mode of liberating and reducing the strangulated bowel. A very natural question, therefore, presents itself, and it is this; do we really possess such views on the subject? It will be found hereafter that we do not; and that, almost as a necessary consequence of this great defect, our present means of accomplishing reduction are far more inadequate to the attainment of so desirable an object, than they might otherwise have been.

Knowing this to be the true state of a subject so highly important, and anxious to remove its defects, I applied myself with much earnestness to its investigation, about nine years ago. At that time it naturally attracted my attention more than at any other, for I was then anxiously engaged in maturing the principles and practice which were afterwards put forth in my work, entitled “*New Views of the Process of Defecation, &c.*,” and which are now so generally adopted; and it appeared to me more than likely, that as these views elucidated the nature and treatment of a great number of intestinal affections, they would throw some light on the causes and effects of strangulation, and eventually lead to a more certain and scientific mode of performing the taxis. These expectations did not prove fallacious; on the contrary, as soon as the corrected views on the circulatory functions of the digestive tube were brought into juxtaposition with certain practical facts, and their mutual bearings carefully considered, all the required preliminary information was ob-

tained, and it then became obvious that it was only necessary to slightly modify the treatment which had proved so successful in other obstinate affections of the bowels, in order to greatly facilitate, if not to insure, the reduction of all strangulated intestinal herniæ, with the exception of those attended by thickening and narrowing of the neck of the sac, strangulating internal bands, and such other states as are either wholly irremediable, or can only be removed by an operation. The modified treatment from which this amount of success was expected, consisted, first, in the introduction of a gum elastic tube into the sigmoid flexure of the colon, and retaining it there while attempts were making at the taxis; secondly, if this failed to diminish the size and tension of the hernial tumour, in attaching the syringe to the tube, throwing up emollient enemata, repeating the same process until the bowels were sufficiently freed of their solid and fluid contents, and then again introducing the tube without attaching to it the syringe.

It remained, however, to put this plan to the test of experience, in such a full and satisfactory manner as to ascertain, as nearly as possible, the extent to which it was either valuable or otherwise. The best means of accomplishing this purpose, appeared to me to be a very simple one, namely, to employ the plan indiscriminately in every case that occurred; to take every opportunity of observing the morbid changes found after death, or during the performance of operations, and in this way learn the due limits of its success, the causes of its failure, and finally, the cases to which it is and is not applicable. Accordingly, adopting this mode of proceeding, I have employed the treatment and seen it employed by others, during the last eight years, in a considerable number of cases, and, as will be presently shewn, with the happiest and most gratifying results. Some of these cases, with an exposition of the principles on which they were treated, appeared in my work on defecation, published in February, 1833; several others were inserted in the *London Medical and Surgical Journal* for

the 13th of August, and the 1st of October, 1836; an additional number have been met with since that period, and two of these being very striking, and having occurred on the same day, were read on the 24th of last March, before the Surgical Society of Ireland, and followed by an oral explanation of the rationale of the treatment, evidently to the entire satisfaction, if not the conviction, of all my auditors. But it would appear that all my efforts have failed in procuring for the practice that full and fair trial, to which it will yet be found to be entitled. For example, although it is strongly recommended in the *Journal des Connoissances Médicales* for October and November, 1833, it does not seem to have ever been employed in France. The same may be said of British practitioners, for none of them have reported on it; and Mr. Lawrence, who mentions it briefly, and rather inaccurately, in the last edition of his invaluable and unequalled work on hernia, does not say that he has tried it himself, or that he has heard of its having been tried by others; and I regret to say that, with the exception of a few gentlemen to be mentioned hereafter, it is equally neglected by the surgeons of this city and country. If I may be allowed to judge, however, this neglect is owing partly to the unsatisfactory manner in which I have, it appears, explained the principles of the practice, and partly to the circumstance of the illustrative cases being published in such a disconnected way, and at such an interval, as to deprive them of much of the force and interest which they would otherwise have possessed.

Such being the actual state of the subject, and seeing the number and nature of the materials now at my disposal, I feel that I only discharge an imperative duty in again bringing it before the profession, and endeavouring to place it in a clearer and more comprehensive and connected point of view.

Commencing, then, with the causes of strangulation in intestinal herniæ, the first consideration is naturally the present state of our information on the matter. To shew this satisfac-

torily, I shall extract Mr. Lawrence's opinions on the subject, as being the most generally received. "That the symptoms of strangulated hernia," he says, "arise from the pressure of the stricture on the protruded parts; and that this cause is not only adequate to that effect, but, indeed, the only one that can be assigned, is too clear to admit of any doubt. Systematic writers have distinguished the causes of incarceration as consisting either in a diminished capacity of the opening, or in the intrusion of additional parts into the aperture. This distinction would not be a very important one, if it were well founded, since the presence of either of these circumstances must imply relatively that of the other. I believe, however, that the former can hardly be admitted as a cause of strangulation. The openings through which herniæ generally protrude being tendinous, cannot contract or diminish in capacity: hence the term *stricture*, equivalent to contraction or narrowing, is objectionable. The parts are increased in bulk, and the ring feels tense, hence it is found to be actually *dilated*, larger, indeed, than in health. The term stricture has led to erroneous practice, to the use of emollients, and such topical remedies as are supposed to possess the power of relaxing, whereas we should attempt to reduce the bulk of the parts. The tendinous openings through which herniæ generally protrude, cannot, by their nature, undergo much change; and particularly do not admit of contraction. The protruded parts, however, are capable of considerable enlargement; and the tendons can produce passively as complete a constrictive effect, as if they had possessed the most unequivocal powers of active contraction. A portion of intestine or omentum, pushed suddenly by a violent effort through the abdominal ring, may be immediately strangulated. A piece of bowel forced down in an omental rupture, a new portion protruded in an old intestinal hernia, or the distention of the contained intestine by its contents, whether of food or air, will so fill up the ring, as to produce incarceration. In all these cases the symptoms cease immediately on reduction, or on the division of the ring, which

92 Dr. O'Beirne *on the Primary Causes of Strangulation*, proves clearly the nature of the cause."—*A Treatise on Ruptures*, 5th Edition, pp. 67, 68. Lond. 1838.

This paragraph, precisely as it stands, has been before the profession for at least twenty-two years, yet it is an undeniable fact, that many intelligent practitioners not only hold, but act upon the old erroneous opinion, that the abdominal rings are active in the production of strangulation. There must be, therefore, some defect either in its matter or its language; and, on submitting it to analysis, this appears to me to be the fact. In the first place, the passiveness of the rings is not insisted upon in sufficiently positive terms, while there is more of assertion than proof advanced in its support. Secondly, the cause of strangulation, in cases where neither omentum nor a new portion of intestine is protruded, is rather left to be inferred than clearly shown. Thirdly, in showing the causes of strangulation in cases of entero-epiplocele, as much is erroneously attributed to the *thickness* of the omentum and the parietes of the intestine, as to the distention of the bowel by its contents. It is most necessary, therefore, to clear the subject from all obscurity or doubt. This I shall now endeavour to do.

It is obvious that the primary causes of strangulation in external intestinal herniæ, exist either in the abdominal openings, or in the parts protruded through them. With respect to the first, as the seat of strangulation, two doctrines are held; one, that the openings are active, the other, that they are passive, in producing this state. These doctrines are evidently founded on the preceding proposition, and, therefore, either one or the other of them must be true, but they are so directly opposite to each other, that either of them must be false, and, consequently, it will be necessary to examine them separately and carefully before a fair decision can be come to respecting them.

The activity of the abdominal openings or rings is advocated by Richter, Schmucker, Callisen, Sabatier, Richerand, Scarpa, Boyer, and many other eminent authorities, all of whom main-

tain, that the effect in question is produced by contraction or narrowing of the rings. But they do not agree respecting the means by which this narrowing is effected, some of them attributing it to spasmodic action of the oblique muscles of the abdomen; others to the elasticity and reaction of the herniary openings; and others to the combined action of both of these causes. The first question, therefore, to be examined, is the power of the oblique muscles to contract the rings. Richter assigned this power to the external oblique, merely from observing the direction and insertion of its muscular fibres; and Schmucker conferred it on the internal oblique in cases where the rings are large and relaxed. But it will be easy to show that neither of these muscles is capable of producing any such effect. In the first place, there is no instance of compression of the vena cava by the aponeurotic opening in the diaphragm through which it passes, or of the femoral artery by the tendon of the triceps adductor femoris; and if those powerful muscles are not capable of contracting their tendinous openings, it seems irrational, in the absence of better reasons than those assigned, to admit either of the oblique muscles to be possessed of that power. Secondly, this theory, or rather, this hypothesis, is only applicable to inguinal hernia, for no one, for instance, acquainted with the anatomical connexions and configuration of the third insertion of Poupart's ligament, which so often becomes the seat of strangulation in femoral hernia, will be disposed to believe that it is or can be acted upon by any of the abdominal muscles. Thirdly, it is a fact that, both before and after reduction, the herniary openings are invariably found dilated instead of being contracted. These facts appear to be conclusive on the point. It only remains, therefore, to consider the elasticity of the rings, and its capability of causing such reaction of their margins as to produce strangulation. Of all the tissues of the animal body, fibrous membrane possesses the very lowest degree of elasticity. Accordingly, when the rings are divided, the cut edges do not fly asunder and enlarge the

wound, as occurs in the division of other tissues; and the consequence is, that it is often necessary to enlarge the incision, before the strangulated intestine can be reduced. Being a physical property, elasticity is as perfect in the dead as in the living body, consequently, the degree in which it is possessed by any of the animal tissues, can be clearly ascertained; and this being the case, it is only necessary to force a finger into any of the abdominal openings, in order to feel and be convinced that it makes no attempt at reaction, either while the finger is retained in it, or after it has been removed. In one of the cases inserted in this paper, the right inguinal ring was operated upon and divided, but, although thus weakened, and of course deprived of such little elasticity as it might have been possessed of, the same ring became again, and soon after, the seat of such obstinate strangulation as to require a similar operation, thereby showing at once, both the nullity of elastic reaction and the agency of some greater power, in producing such a high degree of constriction. Again, if ever the abdominal rings exhibited any thing approaching to elastic reaction of their margins, it is obvious that they should exhibit it when they are but recently forced, and not subjected to distention for more than a few minutes. They are precisely in this state in cases where intestinal herniæ occur for the first time, become, as usual, strangulated, and are soon reduced. But what are the facts? Having met with many such cases, I can answer this question. While the bowel is strangulated, there is every evidence of the herniary openings being more or less dilated, and after the bowel has been reduced, if the hernia be on one side only, it will only be necessary to apply a finger on each of the inguinal or femoral rings, as the case may be, in order to be convinced that the ruptured ring is sensibly wider than that of the opposite or sound side. Finally, it is notorious that the ring of every large and old hernia is proportionably large, and as inactive as one made of any inanimate and inelastic substance. From all these facts,

it is evident that the rings possess scarcely any elasticity, and that they are never narrowed or contracted in cases of strangulated hernia.

Having now proved the abdominal openings to be perfectly inactive in the production of strangulation, the natural conclusion is, that the causes of this state exist in the parts forced through these openings. But the matter to be explained is, how, and in what manner, those parts become the cause of their own firm constriction. To explain this more clearly, I shall commence by showing what occurs in a very simple form of hernia, in which a small double or knuckle of intestine is protruded for the first time, and quickly becomes strangulated. At the moment that such a protrusion takes place, the diaphragm and abdominal muscles, which act alternately and gently in natural respiration, are always acting simultaneously and powerfully. The consequence is, that a current of flatus is impelled with corresponding force into the protruded intestine, which becomes so distended as to be driven, the sac alone being interposed, against the firm margins of the ring, and often with such force as to considerably dilate the latter. The distention is of course resisted by the ring, and, according to the situation of the hernia, by either a strong or a weak fascia; but, below this point, the bowel, being opposed by little more than the common integuments, yields readily, and becomes rapidly enlarged to a size so disproportionate to that of the opening through which it descended, that it is no longer capable of re-passing into the abdomen. There is, at the same time, another cause operating to prevent its return, for the great expulsor muscles, although they soon cease to act simultaneously or with the same force, still act so as to direct and maintain a constant current and pressure of flatus against the strangulated gut. There are reasons for believing also that the bowel is more or less enlarged above, as well as below the ring, so that the latter lies imbedded in a hollow formed by the sac and the intestine. In this way, pressure from within and resistance from without

unite in causing the margins of the herniary opening, although totally inactive *per se*, to produce a much greater degree of constriction of the sac and the intestine, than they could possibly produce, if they really possessed the activity attributed to them by many. It is in this way also, and in this way only, that we are enabled to explain how old and voluminous herniæ become strangulated by their large and perfectly inactive rings, a fact which is, of itself, a strong confirmation of the doctrine which I am anxious to sustain. This explanation of the process under consideration is made much clearer by the following very simple experiment :

After making in a strong common card an opening smaller than that of any of the abdominal rings, pass through this opening a small double or knuckle of human intestine, one of the extremities of which is tied. This being done, let the whole piece of intestine be inflated through its other extremity, and let this also to be tied. The inflated bowel will then be found to have dilated the opening in the card, and, if the inflation be carried to a considerable extent, to have actually ruptured its margin at different points. It will be seen, also, that the gut bulges out above and below, and considerably beyond the margin of the opening, and the sharp edge of the latter lies imbedded in the intestine, and constricts it as firmly as if it were tied by a cord.

In my clinical lectures on strangulated hernia, this experiment has enabled me to convince a large and intelligent class of students, not only of the truth of the preceding views, but also to satisfactorily explain the precise condition in which the strangulated bowel is placed. I shall have occasion, therefore, to refer to it in treating, as I shall now proceed to treat, the latter very important portion of the subject.

The first question connected with the condition of the strangulated intestine, is one of the very highest importance. It is this: when a small double or knuckle of intestine becomes completely strangulated, does air pass into and out of the bowel

so constricted, or, in other words, is there any communication between the cavity of the constricted bowel and that of the bowel within the abdomen? This question, strange as it may appear, has never been raised or considered by any author, ancient or modern, with the exception of my friend, Mr. Geoghegan, who says, that “the intestinal tube, compressed by the sac, cord, and tendon, is rendered impervious.” But I shall now show that this is not the fact. The smallest double or knuckle of prolapsed intestine consists of a middle portion, or body, and of two extremities, one corresponding or connecting itself with the stomach, the other with the cæcum. These extremities always occupy the hernial opening, and, when distended, lie there like two parallel tubes. Consequently, when we consider this circumstance in connexion with the smallness of the ring, and the thickness of the walls of the doubled intestine, it becomes evident, that if there be the communication in question, it must be an extremely narrow one. But to come at once to the point: no proposition in nature can be clearer or more simple than this; namely, *that, as strangulation is produced by a distending force acting from within outwards, and separating the parietes of the constricted gut, and maintaining them at a greater or less distance from each other, there must be such a communication.* In fact, we must either deny the premises, or admit the conclusion; and few will question the former. But if there be any obscurity in this reasoning, it will be instantly removed by performing the simple experiment already described, and by then observing the state of the tied extremities of the intestine above the opening in the card. It is true, as has been already shown, that this channel of communication is an extremely narrow one, but we know that the subtlety of air enables it to permeate the minutest possible opening; and consequently, that although neither solid nor fluid feces can enter, flatus can pass freely into and out of the smallest strangulated hernial tumour. Upon the same principles, the same may be said of those cases in which a small portion of

the circumference of an intestine is, as it were, nipped or pinched up, and becomes strangulated, with this difference, that the tumour communicates with the intestinal cavity but by one and a much smaller opening. In cases of entero-epiplocele, it is obvious that the force with which the intestine is distended, will be sufficient to compress the soft and yielding omentum so as to make it occupy a smaller space, and, at the same time, to preserve at least a small channel of communication. We now see why recent and small strangulated intestinal herniæ always contain flatus, very rarely fluid feces, and never solid fecal matter. If, then, such a free passage exists in strangulated enterocele and entero-epiplocele of small size, it is plain that it must also exist in those of greater volume, in which the rings are so much larger, and permit the descent of both fluid and solid feces. There are, however, some exceptions to this general rule, for all the parts contained within the hernial foramen may be attacked by inflammation, and become so enlarged, thickened, and matted together, as to compress the bowel, and render it quite impervious; and the same effect may be produced by the descent of a large additional portion of omentum, of intestine, or of both; by constriction of the neck of the sac, and by other secondary causes of strangulation.

Advancing from reasoning and experiment to proofs of a higher order, I shall now illustrate the existence of the communication in question by some practical facts.

1st. It is generally very difficult, and often impracticable, to divide the stricture on the outside of the sac, but we seldom find any great difficulty, and never fail, in dividing it when the sac is opened. With respect to the first of these facts, it may be explained thus. The sac, being acted upon by a distending force, from within, and a resisting force from without, is almost inseparably joined to, and fixed at the ring; the rest of the sac being also acted upon by a distending force from within, and having interposed between it and the intestine a serous fluid, which offers more or less resistance to this force, becomes

almost as firm and incompressible as the most solid body. It is obvious, therefore, that, from its fixation at the ring, and its firmness and incompressibility, the tumour can, with difficulty, be drawn in any direction, or its contents acted upon in any way, so as to enable either a director, the button of a bistoury, or the point of a finger to be introduced. But let me now take a case of strangulated enterocele, as one in which the difficulty under consideration is frequently encountered, and let me ask the question, if, in such a case, the bowel were impervious either at or above the ring, would such difficulty occur in dividing the stricture on the outside of the sac? Certainly not; for the bowel can only become impervious by its parietes being compressed together, a state of the parts the very reverse of that necessary to distend the ring: and although a quantity of flatus may be intercepted and confined in the lower part of the bowel, it could not possibly produce such a state of the hernial ring and tumour, as is met with in cases of strangulated enterocele. Here is one of the practical proofs of the existence of the communication for which I contend. With respect to the second of these facts, namely, the comparative facility of dividing the stricture after the sac has been opened, the distended intestine can then be acted upon directly, and by making pressure on it close to where it is embraced by the ring, the contained flatus passes upwards and downwards, and so as to leave sufficient space for the introduction of the director, or the point of the finger; yet even under all these advantages, there is always some little difficulty in introducing either. But if the bowel were impervious at or above the ring, no difficulty whatsoever of the kind would be met, for as I have already observed, the parietes of that part of the gut would then be collapsed and compressed together, and consequently the ring, instead of being distended, would be more or less open. This is another proof in point.

2nd. The local application of ice and different refrigerating mixtures is frequently employed, chiefly with the indication of

condensing the flatus confined in the strangulated bowel, and thus diminishing the size of the tumour, so as to render it more reducible ; but these local applications rarely succeed, because as soon as any quantity of the confined gas becomes condensed, so soon is the space thus created occupied by an equal quantity descending through the canal of communication. This proof is irresistible.

3rd. It will be found that the strangulated hernial tumour can be completely emptied of its gaseous contents, without either puncturing or cutting into the distended intestine, or dividing the structure, a deed which it would be totally impossible to accomplish, if the bowel were completely impervious either at or above the ring.

Having exhibited the remarkable state in which the intestine is placed, I have now to show the condition of the rest of the intestinal tube, in cases of this description. After death we generally find the whole of the large and small intestines within the abdomen in a dilated state ; during life, I have ascertained that the rectum is more firmly contracted than usual ; and there appears to be no reason why this should not be considered as their ordinary condition in the living subject, while labouring under strangulation.

Having before us this view of the strangulated and other portions of the intestinal tube, we are enabled to see at once, that, in such cases, various forces oppose our attempts at the taxis, and that these forces are as follow, viz.: the great size of the hernial tumour, compared with that of the ring—its almost immoveable attachment to the latter—its great firmness and incompressibility, its enlargement above as well as below the stricture—the constant current and pressure of flatus into the constricted and distended intestine—the acute angles formed by the convolutions within the abdomen, and immediately above the stricture, and the extremely unyielding nature of the ring itself. The consideration of such numerous, various, and powerful forces acting in opposition to our attempts at the taxis, is well calcu-

lated to show the difficulties and uncertainty in effecting reduction ; the injury likely to be done by using much force ; and the little power which the hands, however judiciously applied, can exert over a tumour so situated, It is true, that notwithstanding the opposition of all these forces, such attempts have often prove successful, but I apprehend that in such instances, their success was more owing to an occurrence not observed or inquired into, and which I shall notice hereafter, than to any peculiar tact or manipulation on the part of the operator. But the question which we have now to consider is, how are all these forces and difficulties to be overcome? We see that they are all but the products of distention of the strangulated intestine by flatus, and we know that we can evacuate this flatus by dividing the stricture, or by puncturing or cutting into the intestine itself. But as our object is to avoid the first, while the two last can only be thought of in an argument, the very simple question suggests itself, can we, by any other means, empty the bowel of its gaseous contents? This naturally leads us to survey the state in which the whole of the intestinal canal is placed, and we find, first, that the cavity of the strangulated gut is not obliterated, but permits air to pass freely into and out of it ; secondly, that all that portion of the small intestines within the abdomen, and the whole of the cæcum and colon, are dilated, and also afford free passage to air ; thirdly, that the rectum is firmly contracted, and that it alone opposes the exit of this elastic fluid. The instant that we obtain this view of the facts, it becomes manifest, that the great object in view may be quickly obtained, merely by introducing a gum elastic tube through the rectum, into the colon, and retaining it in that situation until the flatus contained in the large and small intestines, and ultimately that in the strangulated intestine, is completely evacuated, and the tumour so diminished in size and tension, as to start up into the abdomen, as it often does, *proprio motu*, or be easily reduced by the taxis. But to complete this view, it is necessary to consider the circumstances which may occur, either to cause

the complete failure of this mode of assisting the taxis, or merely defeat the first attempt at its employment, and afterwards prevent its complete success. If the strangulated bowel be rendered impervious by any of the causes which I have already mentioned, it is clear that the introduction of the tube cannot possibly have the effect of emptying the hernial tumour; but as the existence of these causes cannot be ascertained beforehand, the fact only shews the propriety of employing the practice in all cases indiscriminately. If, previous to, or during strangulation, the bowels happen to be loaded with solid or fluid feces, the lodgments of fecal matter in the cæcum, the lower portion of the ileum, and the sigmoid flexure of the colon, will necessarily prevent the passage of flatus, and prevent the success of the first introduction of the tube; but this circumstance only shews the necessity of persevering in the practice, assisted by repeated enemata, in order to eventually succeed.

So much for the principles of the practice. It remains that I should shew how far the former are supported by the success of the latter. To do this in such a manner as cannot fail to fix the attention and acquire the confidence of my medical brethren, I am compelled to collect into one general view the whole of the cases, published and unpublished, successful and unsuccessful, in which the practice has been employed by myself and others during the last eight years, and I shall now insert them, as nearly as possible, in the order of their occurrence.

CASE I.—In November, 1830, an old man who lived in a wretched and filthy hovel in Hanover-square, off Hanover-lane, was attacked with strangulated hernia. The hernial tumour was situated a little below the umbilicus, and had resisted all attempts at the taxis. The case was seen by Dr. O'Beirne and Mr. Robert Adams, in consequence of considering it one that would require operation. In consultation, the former gentleman proposed to try the effects of enemata, given through the usual gum elastic tube. His bowels had not been moved

for several days. I introduced the instrument myself with great ease. On arriving at the sigmoid flexure of the colon, a considerable quantity of flatus and fluid fæces flowed through the tube ; the patient expressed himself as feeling greatly relieved ; and on examining the situation of the hernia, the tumour had completely disappeared. I next threw up (without withdrawing the tube) a strong cathartic enema, which, in a few minutes, produced a copious discharge of solid and fluid fæces. On the following morning he resumed his usual occupations.—
(*Communicated by G. T. Hayden, Esq. A. B., M. R. C. S.*)

CASE II. Mary Bentley, twenty years of age, admitted at ten o'clock in the morning of Tuesday, August the 16th, 1831, into the Charitable Infirmary, Jervis-street, under my care. On the right side, immediately below the pubal attachment of Poupart's ligament, and slightly ascending over this ligament, she has a tumour, which is as large as a walnut, tense and painful, yet not discoloured. It imparts all the feel of an entero-epiplocele, and is attended with the following symptoms, viz., anxious and painful expression of countenance ; jactitation, nausea, vomiting ; constipation of four days' duration ; pain on pressure of the abdomen, particularly over its umbilical and hypogastric regions ; tongue brown at its base and along its centre ; great thirst and heat of skin ; pulse 100, hard and contracted. She states that at 9 o'clock on Sunday morning the 14th, she had been pumping water to supply her master's house ; that, at two o'clock in the afternoon she was seized with sickness of stomach and violent pain in the belly ; that she vomited repeatedly during the evening, retired early to bed, and there, for the first time, discovered a tumour in her groin, and pain and difficulty in extending the right thigh and leg. She adds that she went the following morning to the next apothecary, who gave her some pills and an injection, but without affording any relief. The apothecary she mentions had the humanity to attend her to the hospital ; and this gentleman states, that, suspecting the nature of the case, he gave her pills of calomel and cathartic

extract, and a tobacco enema, and that he could not be deceived in asserting that he saw her vomit stercoraceous matter. It does not appear, however, that she has had what could be considered as fecal vomiting, since her admission into hospital.

Blood, which was afterwards found highly buffed and cupped, was now taken from the arm by a large orifice, and until she became very faint. The taxis was then tried by myself, and subsequently by my colleague, Mr. Adams, but without the least success. We agreed that a tobacco enema should be administered as soon as possible, and if it also failed, to lose no time in performing the operation. In the mean time I mentioned to Mr. Adams my peculiar views respecting strangulated hernia, and proceeded to pass the tube into the colon. The moment this was accomplished, a considerable quantity of flatus escaped through the tube, and on quickly laying my fingers flat on the tumour, I found that it was diminished by at least three-fourths of its former size, and that it gave the feel of containing omentum only. Mr. A. and several pupils were present, and satisfied themselves of the reality of this important change in the hernial tumour. In a few minutes all nausea and vomiting ceased, the pain she felt was inconsiderable, and she was directed to have, as soon as possible, a mild enema administered by the tube. In less than half an hour after, the tube was again introduced, and gave exit to about half a pint of fetid fluid feces. The enema was then thrown up, and she was ordered to have ice applied to the groin. Three o'clock, P. M., tumour very small and inelastic; slight tenderness of abdomen; bowels not freed; pulse 92, full and hard. Directed to have the tube introduced, another enema to be administered, and to take two pills composed of five grains of calomel and five grains of compound extract of colocynth. Ten o'clock, P. M., has had several copious fluid evacuations within the last three hours, and she is now greatly relieved.

Wednesday, 17th, nine o'clock, A. M. Slept well last night,

and has had several free motions. The abdomen is now rather tender to the touch; tongue cleaner; pulse 98. Thirty leeches to be applied to the abdomen; directed to take a wine glassful occasionally of a mixture composed of equal parts of infusion of senna and infusion of quassia. Seven o'clock, P. M., rather weak, but has no tenderness of the abdomen.

Thursday, 18th, nine o'clock. Reports herself as feeling perfectly well in all respects. In the course of a few days, a truss was applied, and she was discharged free from all complaint.

CASE III.—On the sixth of May, 1832, Mr. Fea, a surgeon of this city, requested my assistance in the case of David Kearney, a poor man residing in a stable lane, at the rere of Summer-hill, and sixty-five years of age. On seeing him, I found that he had a large, tense, and rather painful scrotal hernia on the left side; and he stated that he had been afflicted with this rupture for thirty-five years, but that he could always reduce it until four days before. His abdomen was swollen, tympanitic, and rather painful on pressure; his pulse 80, full, and intermitting; his tongue moist and rather clean; his bowels were obstinately constipated, and he had nausea, but no vomiting. Finding that Mr. Fea's attempts at the taxis had failed, and having satisfied myself that all such attempts were fruitless, I recommended the tube to be introduced into the colon. This was immediately done, but no flatus or feces escaped; no visible change occurred in the state of the hernial tumour, and, on withdrawing the instrument, some inches of its upper extremity were coated with solid feces. Shortly after the tube was again introduced, and through it a fetid turpentine enema administered. In a few minutes, he discharged a quantity of solid feces and some wind, but without producing any impression on the tumour, or any alteration in the general symptoms. Under these circumstances, I advised his immediate removal to the Richmond Surgical Hospital, and gave an order for his admission; but being obliged to go some distance from town, I was

under the necessity of placing him under the care of my colleague, Dr. M'Dowel, who found it necessary to perform the operation soon after his admission, and has favoured me with the following particulars. On exposing the strangulated portion of intestine, the longitudinal bands and the appendices epiploicæ, the latter remarkably fatty, left no doubt of its being formed by the colon; anteriorly, its surface was found to be considerably congested, but posteriorly and externally, it was adherent to, or rather identified with the sac, and its cavity so stuffed with hardened excrement, that it could not be emptied without dividing the ring to a considerable extent. This being done, the excremental contents were pushed up, the protruded intestine was left in the scrotum, the parts were brought together by five points of suture through the skin, and the patient was placed in bed, with directions to have immediately thirty drops of tincture of opium, and an ounce of cinnamon water, as a draught, and also an enema consisting of half an ounce of oil of turpentine, one ounce of oil of olives, one ounce of compound tincture of senna, and ten ounces of warm water.

7th. Eight o'clock, A. M. Has been restless and in pain, and has had no motion during the night. The abdomen is now very tense and tender, and he complains much of pain in the scrotum; he has no thirst, nausea, or vomiting; the pulse is 78, regular, and soft; the tongue is clean; and there is rather a defined fulness above the pubes. Catheter introduced, and a pint of high-coloured urine drawn off. Ordered to have weak beef tea *ad libitum*, to take a draught of castor oil, compound tincture of senna and cinnamon water, to repeat this draught in four hours, if necessary, to have an emollient enema immediately, and a poultice to be applied over the scrotum.

8th. Copious discharges from the bowels; tongue white and slightly furred; pulse 102, and feeble; slight singultus; constantly sleeping, or rather in a state of stupor, and difficult to be roused. A blister applied between the scapulæ.

9th. Cold and insensible; pulse scarcely to be felt; respira-

tion twenty-five in a minute ; mucous rale in the trachea ; frequent singultus. Died about noon.

The impression which the state of the patient subsequent to the operation, made upon Dr. M'Dowel's mind, was, that he laboured under the effects of constitutional irritation, and not under those of inflammation. He acted accordingly, and the post mortem examination showed that he was right, for no inflammatory or other morbid appearance could be discovered within the abdomen.

CASE IV.—On the 24th of July, 1832, I accompanied Dr. Maxwell Fleming and Mr. Richard Morrison, both surgeons of this city, to see Mary Dunne, residing at the North Strand, and aged 30. We found her vomiting frequently, very restless, and complaining of intense pain all over the abdomen, particularly about the umbilicus. Her belly was tense, and painful on pressure ; her countenance anxious ; her pulse quick, small, and hard ; her tongue foul ; and her bowels had been obstinately constipated for several days. She stated that, two days before, she had felt something gave way in her groin, on making an effort to lift a heavy weight, and that she soon afterwards felt sick at the stomach, pain in the belly, and a painful swelling in the right groin. On the right side, there was a femoral hernia, which Dr. Fleming had previously failed in reducing ; and in front of the hernial tumour, which was of a moderate size, and felt as if it contained intestine only, there was an enlarged and rather painful inguinal gland. I recommended the tube to be introduced, and if doing so failed in causing the hernia to return or in rendering it reducible, to throw up a tobacco enema. It so happened that circumstances prevented me from waiting to see these means ; employed but on returning in less than hour, Dr. Fleming and Mr. Morrison informed me that no flatus or feces had escaped through the tube, that its introduction was followed by no change in the hernial tumour, that accordingly an injection, composed of half a drachm of tobacco and a pint of warm water, had been thrown up, and

that not finding any favourable change, another of the same description had been administered. But I soon perceived the cause of failure, for the tube used on the occasion was not one of the description that I employ. It was one of those which have been manufactured here, and are considered an improvement on mine. For the purpose of rendering it more portable, it was made of two gum elastic tubes, joined by a brass male and female screw, so as to form a tube of a foot in length, and having at its centre a projection of brass sufficient to effectually prevent either of its ends from being introduced to a greater height than five and a half or, at the most, six inches, without using great violence, and inflicting serious injury on the rectum. Accordingly, it actually was not introduced higher. I now examined the hernial tumour, and it appeared to me to feel less tense; but, be this as it may, very little effort enabled me to reduce the intestine, which retired with the usual gurgling sound, and the patient felt at once greatly relieved. She continued to vomit, complained of pain in the head and of great weakness, felt exceedingly cold, and her pulse was very weak and slow; but these were obviously nothing more than the specific effects of tobacco. Two more enlarged inguinal glands were now felt in the situation which the hernial tumour had occupied, and as these were very painful, she was immediately sent to the Richmond Surgical Hospital, and placed under my care. In a few hours after admission into hospital, eighteen ounces of blood were taken from the arm, a turpentine enema was administered, she was directed to take, every third hour, a pill composed of two grains of calomel and three grains of compound extract of colocynth; two dozen of leeches, and, afterwards, a large emollient poultice, were ordered to be applied to the enlarged glands. The bowels were soon freely moved, the glands gradually returned to their natural state, and after applying a proper truss, she was discharged from hospital on the 4th of August following.

CASE V.—On Saturday the 15th of December, 1832, after

prescribing for a gentleman confined in the Four-Court Marshalsea, I was requested to visit Mr. John F———l, who was said to be very ill in one of the rooms of the prison. On seeing him, he appeared to be under thirty years of age, and very robust ; his countenance was pale and expressive of acute pain, and his pulse quick, hard, and rather small ; he complained of severe pain in the abdomen, stated that his bowels had not been freed for several days, and pointed to the hearth, where, he said, he had just vomited up the contents of his stomach. After his friends had retired at his request, he informed me that he had been afflicted with rupture for several years ; that he had never worn a truss ; and that although the bowel had frequently descended, it was always easily returned until early that morning, when he failed in reducing it as usual. On examination, I found that the abdomen was tense and painful on pressure, and that he had a moderate sized inguinal hernia on the right side. The hernial tumour was very tense, particularly at the ring, slightly suffused with redness, and seemed to be altogether formed by prolapsed intestine distended by air, and, as far as the touch enabled me to judge, containing a small quantity of feces. Immediately after ascertaining the nature of the case, the patient was placed on a sofa, in the usual position, and attempts were made at the taxis. These attempts were made four or five times, at distinct intervals, and with as much force as could be safely employed, or as he could bear, for he complained of the severe pain which they gave him ; but I found it impracticable to reduce the hernial tumour, and improper to persevere longer in efforts for that purpose. Finding that I had failed, he informed me that he had sent for a neighbouring apothecary to take blood freely from his arm, and apply cold lotions to the scrotum, as these means had saved him from an operation when he was first ruptured, and he earnestly intreated me to adopt the same practice. But I succeeded in inducing him to wait until I could procure a tube and try its effects. Accordingly a messenger was despatched to my house for the neces-

sary apparatus. In the meantime, the apothecary, Mr. Carroll, of Meath-street, arrived, and was requested to go for a purgative injection, composed of a pint of the common enema, an ounce of oil of turpentine, and the same quantity of sulphate of magnesia. As soon as this and the apparatus were procured, the patient was turned on his left side, with his knees drawn up, and the tube, which happened to be a stomach tube, after being oiled, was passed up the rectum, without much difficulty, to the height of about ten inches. No flatus could be heard escaping, but Mr. Carroll, who assisted me, on placing his hand close to the lower extremity of the instrument, said that he distinctly felt a current of air passing from it to his hand; and, on looking at the hernial tumour, we both agreed that it was sensibly diminished in size: the patient also said that he was certain of the fact, and assured us that he felt himself considerably relieved. This appearing to be a favourable moment for attempts at the taxis, they were renewed, but proved ineffectual. Considering the failure to arise from the eye or opening in the side of the tube having become blocked up with solid fæces, I removed the instrument, and found that I had judged correctly. The tube was now well washed and syringed out with cold water, and after being dried and then oiled, was again introduced, and passed up to a greater height. Immediately a still greater reduction took place in the hernial tumour, and, on making another attempt at the taxis, the tube still remaining within the bowels, it succeeded, and the strangulated intestine was completely reduced. The patient immediately felt himself freed from all pain and distress, but as the sigmoid flexure was evidently loaded with solid fæces, it was considered necessary to throw up the enema which had been prepared. His bowels acted freely soon after, and, on visiting him the following day, I found him perfectly well, but lying in bed, as he was directed, until a proper truss could be applied.

On asking him the cause of his not having worn a truss, he stated that a surgeon had advised him to do so, but that he

was afterwards dissuaded from it by a physician, whom he named. Knowing the high character of the latter gentleman, I endeavoured to discover the reason of his having objected to the use of a truss, but the only circumstance which seemed to explain the matter, was an impression which appeared to be strongly entertained by the patient, namely, that his right testis had always ascended with the gut into the ring, and remained there. If such a circumstance had occurred, it would perhaps constitute a valid objection to the use of the truss; but I satisfied myself that, on this occasion, both testes, although remarkably small, particularly the right, were in the scrotum, and consequently, that no such objection existed against wearing so necessary an instrument.

CASE VI.—Mary Jones, weakly, aged 69, and of costive habit, admitted into the Richmond Surgical Hospital, late on the 10th of February, 1833, with strangulated femoral hernia. Every known means of relieving her was tried, and amongst others, the gum elastic tube was introduced up the rectum and into the colon, but without causing an escape of flatus, or making any impression on the size, tension, or pain of the tumour, or upon the constitutional symptoms. While the instrument remained in the bowels, I passed my finger along its side, and distinctly felt the instrument through the thickness of the parietes, and to the left of the rectum. This woman refused to submit to an operation, died on the 13th, and on a post mortem examination, it was found that the sigmoid flexure was uninjured, contained no solid or fluid feces, and very little gas; and that its lower half hung into the pelvis, doubled over and to the left of the rectum. The descending colon was also empty and contracted throughout: it was embraced and firmly constricted, at its commencement, by an old organized band of coagulable lymph; and, on the cœcal side of this band, the intestine was distended by such a mass of solid excrement, that even intestinal gas could scarcely pass downwards. The strangulated portion consisted of a loop of the upper part of the ileum; and

of course, its gaseous contents could not be evacuated, in consequence of the obstruction which existed in the descending colon. In this case, the sigmoid flexure could not have been otherwise than in an empty state, during life ; while the circumstances attending the introduction of the tube, leave no rational doubt of this portion of the colon having occupied the same situation before, as after death.

CASE VII.—Mrs. Catherine Hart, of No. 14, Chatham-street, aged 47, was attacked on the 6th of January, 1834, with severe vomiting ; great pain and distention of the abdomen ; pain in the groin ; thirst ; sinking of the countenance ; and her pulse was 120, and small. In this state she was seen by Dr. Ireland, who discovered that she laboured under strangulated femoral hernia, of small size, intestinal, and on the right side. She was soon after seen by Mr. Hayden. These gentlemen employed the taxis frequently ; two tobacco injections were administered by the common bag and pipe, but without success, and preparations were made for operating. They now requested my assistance ; and, on my arrival, I found, in addition to an aggravated state of all the symptoms, that the face and extremities were cold, the pulse exceedingly feeble and small, and an alarming degree of general prostration. I now tried several times to reduce the hernial tumour, but all my efforts failed ; and at once I agreed with Dr. Ireland and Mr. Hayden, that the operation should be immediately performed, if the introduction of the tube did not succeed. Accordingly, with their free permission, I introduced the tube. When it reached the height of about ten inches some flatus began and continued to escape ; the tumour became very sensibly diminished in size, and less tense. The taxis was now employed, and after using very slight pressure, the gut suddenly darted up into the abdomen ; the patient instantly expressed general relief : the pulse became fuller and stronger ; the countenance and general appearance remarkably improved ; and every thing like nausea ceased. Ordered to lie on her back,

with her knees drawn up, and to have an enema, and two pills of calomel and cathartic extract. This woman was quite well in two days after, and a truss was applied."

CASE VIII.—"Samuel Hodgens, aged 17, a sailor, admitted on the 13th of June, 1834, into Jervis-street Hospital. He had a small, oblique, inguinal hernia on the right side. It was very tense and painful to the touch. He could not bear the slightest pressure on the abdomen. He retched frequently, had hiccup; vomited up every thing he drank. His countenance was anxious; pulse exceedingly quick and small; skin hot; tongue white and dry. The hernial tumour was so very painful and tense, that he could not bear even the slightest attempt to reduce it. The tube, therefore, was introduced, and an enema thrown up; the tumour became sensibly diminished in size and tension, and was eventually easily reduced; all alarming symptoms disappeared: and the man was discharged perfectly recovered."—(*Communicated by Mr. Cullen, M.R.C.S.L., and Resident Surgeon-Apothecary of Jervis-street Hospital.*)

CASE IX.—"James Berbeck, of Moore-street, aged 55, admitted on the 12th of October, 1834, into Jervis-street Hospital. On admission, he had a very large scrotal hernia, perfectly solid, and receiving no impulse from coughing, and irreducible by the taxis. The same treatment was employed; and after the bowels discharged an enormous quantity of feces, the tumour diminished considerably in size, was easily reduced, and the man left the hospital, free from all complaint, on the following day."—(*Communicated by Mr. Cullen.*)

CASE X.—"John Lawless, of Fishamble-street, aged 26, admitted into Jervis-street Infirmary, the 28th of December, 1833. He had just received a wound of the abdomen, two inches long, and at the right side of the umbilicus. A portion of the intestine, with a little omentum, had protruded through the wound, and was quite hard, very painful to the touch, and

of a pink colour. It could be easily passed into a space between the sheath of the rectus abdominis muscle and the peritoneum, but not into the cavity of the latter. In this case, the tube and enema, together with bleeding resorted to twice and freely, enabled the protruded gut to be returned, without enlarging the wound. This man died from a wound of the stomach, about half an inch long, and situated about two inches from its pyloric extremity.”—(*Communicated by Mr. Cullen.*)*

* During five campaigns in the Peninsula, France, and Belgium, I had frequent opportunities of seeing cases of traumatic abdominal hernia. In all of them there was considerable difficulty in returning the protruded bowels, and it was often necessary to enlarge the wound for that purpose. I do not recollect an instance in which the intestine was not greatly distended, even in cases where examination after death proved it to be wounded. While on this subject, I shall mention a singular circumstance which occurred during the memorable retreat of the British Army from Burgos, in 1812. When we came close to Salamanca, I was directed to visit a wounded artillery driver who had been just brought into that city. On seeing him he was labouring under all the symptoms of strangulation, and I found a large bunch of very distended and highly inflamed intestine protruding through a wound in the abdomen. I proceeded at once to reduce the bowel, but to my amazement, found the whole of it encircled and firmly tied close to the abdomen, by a strong common thread. My first step was to attempt to introduce a director under this thread, but I found it impossible to do this without injuring the gut, and had to cut fibre by fibre cautiously until the thread was cut through. This being done, I ascertained that the wound was about an inch and a half long, and had passed through the left rectus muscle, about five inches above the os pubis. With much difficulty I reduced the protruded parts, taking care to satisfy myself that they were not pushed into the sheath of the rectus, but into the abdomen. The lips of the wound were then brought together by three points of suture through the integuments; the intervening spaces were supported by adhesive straps, and compresses and a roller applied. This being done, I took more than twenty ounces of blood from his arm, and had an enema given to him, which operated quickly and freely. He was now greatly relieved, and I placed him in an hospital, with directions that he should lie with his thighs and head raised. At this time the enemy was rapidly coming on, and as either another battle on the same field, or a retreat into Portugal was momentarily expected, I was obliged to leave him to his fate. What became of him I never could hear, but have no

CASE XI.—Anne O'Reilly, aged 73, admitted into the Richmond Surgical Hospital on the 13th April, 1836, with strangulated femoral enterocele on the right side. She states that on making a great effort at stool that morning, her bowels having been constipated for two or three days before, she perceived, for the first time, a tumour to protrude at the right groin. Attempts at the taxis, the tobacco enema, and one introduction of the tube having failed in either giving relief or effecting reduction, the operation was performed, and the patient recovered.

CASE XII.—Catherine Carey, aged 73, admitted into the Richmond Surgical Hospital on the 27th April, 1837, with strangulated entero-epiplocele on the right side. The taxis was employed, and a portion of the hernial tumour was reduced, but another portion remained in the ring, and the patient was not relieved. I do not recollect having seen this case, but I find the following passage in the case-book of the hospital, viz. "After the taxis was employed, an enema was attempted to be given with the tube, but the latter could not be passed up more than two or three inches, and the injection returned almost immediately without bringing away any fecal matter." The operation was performed, but, in the course of a few days, the patient died. The appearances observed during the operation and after death, were of a very peculiar and interesting description, and will, no doubt, be published by the gentleman who had the case under his care. I consider, therefore, that I ought not to intrench upon his province by here detailing the particulars of these appearances; but I can safely assert, that whenever they

doubt that he was kindly treated and attended by our gallant enemies, who, soon after, entered and took possession of the city. This unfortunate man acknowledged to me that he had been marauding in a village near to the city; that, in a scuffle, one of the inhabitants wounded him in the belly with a knife, and that the village barber, under the pretence of curing him, but evidently, with the most fiendish malice, had applied the ligature which I removed. So true is it that

"There are more things in heaven and earth,
Than are dreamt of in our philosophy."

are published, they will be found such as to account for the failure in introducing the tube sufficiently high up, and show the physical impossibility of saving the patient by any mode of treatment that human ingenuity is capable of inventing.

CASE XIII.—John Hamrock, aged 78, very low sized, emaciated and feeble, had a small inguinal hernia on the right side, which was always reducible, and for which he had never worn a truss. On the morning of the 5th of January, 1838, upon making some unusual exertion, he felt pain in the hernial tumour, which he endeavoured to reduce as usual, but failed in doing so. Shortly after, he complained of nausea, and the tumour became much more painful. Towards evening he was seized with vomiting and severe pain in the abdomen, became restless, and in this state passed a night of great suffering. On the following morning, he applied for relief at the Richmond Surgical Hospital, and was admitted under my care. On admission, his countenance was very pale, sunken, and expressive of great suffering and anxiety; his pulse quick, small, and intermitting; he vomited frequently; his abdomen was distended, tympanitic, and painful on pressure; and the hernial tumour, which extended nearly to the testis, was elastic, of a bright red colour, extremely tense, and very tender to the touch. It appeared also that his bowels had not been freed for some days.

Under these circumstances, no time was lost in putting him in bed, placing him in a proper position, and employing the taxis. In the course of nearly an hour, five different attempts at reducing the intestine were made, with exquisite pain to the patient, but without affording him any relief, or making any change in the state of the tumour. The gum elastic tube was now passed up the rectum to the height of ten inches, and through it were thrown up a pint and a half of warm water, two ounces of castor oil, the same quantity of olive oil, and an ounce of sulphate of magnesia. When the whole of this enema was injected, he requested to be allowed to go to stool; the tube was accordingly withdrawn, and he passed a considerable quan-

tity of flatus and fluid feces, with some slight relief to his sufferings. On examination, also, it was found that the tumour was not quite so tense or painful to the touch; and attempts at the taxis were again made, but ineffectually. The tube detached from the syringe was again passed up the rectum to the same height, and while there, the tumour was examined and found considerably reduced in size and tension. The taxis was, therefore, again employed, and with immediate success, for the first effort succeeded in reducing the whole of the prolapsed intestine, and in instantly freeing the patient from all the symptoms of strangulation. A truss was immediately applied, and an enema of the same description as that previously administered thrown up through the tube, which still remained in the rectum. During the course of the day he had several solid stools. On the following morning, he expressed himself as feeling perfectly well, and was discharged at his own request.

“ Rutland Square, 7th January, 1838.

CASE XIV.—“ DEAR O’BEIRNE. The circumstances of the case of strangulated hernia I mentioned to you to-day are chiefly as follow : I was called upon yesterday to meet Mr. Ribton, in consultation, upon a recent case of strangulated ventral hernia, immediately above the umbilicus, in an elderly gentleman. It was attended with the usual symptoms of constipation, vomiting, and extreme restlessness. All attempts to reduce the hernia, which was very small and elastic, indicating the presence of intestine, proved unavailing, although blood-letting and the usual remedies had been employed. I suggested to Mr. Ribton your practice of the introduction of the tube, with the view of affording exit to the flatus, and then to seize the favourable opportunity of reducing the intestine by the taxis. The plan proved eminently successful. Mr. Ribton passed a long tube fairly into the colon, gas was heard to pass, and by efforts similar to those which before proved unsuccessful, the intestine was easily re-

duced. Had this manœuvre failed, we had every thing prepared to proceed to immediate operation.

“ This case occurring on the same day with that under your own care in hospital, must be very satisfactory to you, as they both afford an illustration of a point of practice of the highest importance, not generally known, I believe, to the profession, but with which it ought to be speedily made acquainted.

“ I am, &c. &c.,

“ RICHARD CARMICHAEL.”

CASE XV.—James Lenehan, an old pensioner, admitted into the Meath Hospital, on the 13th of May, 1838, and labouring under strangulated hernia, states that the hernial tumour came down frequently, but was always reducible by himself. On the last occasion he was not able to reduce it, and, at the time of his admission into hospital it had been unreduced for nearly four days. After admission, frequent attempts at the taxis were made, but without success. The tube was now introduced, to the height of two feet, into the colon by the resident apothecary, Mr. Parr, and a turpentine enema injected through it; the bowels were soon after freed of their contents, and, in the space of an hour, the hernial tumour was found to have disappeared, but how soon its return occurred after the introduction of the tube, or the evacuation of the bowels, was not ascertained. Be this as it may, the patient was found free from all complaint, when the situation of the tumour was examined. Shortly after, the Surgeon-General, Mr. Crampton, who was sent for, and requested to come prepared to operate, arrived at the hospital, and had merely to order the application of a truss. The patient was discharged well, on the fourth day from his admission.—(*Extracted from the Journals of the Meath Hospital.*)

CASE XVI.—“ Mary Hughes, an old clothes-vender, residing in John's-lane, aged 60, and of full habit, admitted under my care into the Charitable Infirmary, Jervis-street, at

half-past three o'clock, P. M., on Saturday, the 30th of June, 1838. She had constant stercoraceous vomiting; her features were sunken; her pulse was quick, small, weak, and easily compressed; the abdomen tender to the touch, and, on examination, a small femoral hernia, evidently containing omentum and intestine, was found in the left groin. It appeared that she first observed the swelling in her groin about twelve years ago; that she never paid the least attention to it, it being no inconvenience to her, until the preceding Tuesday, when the usual symptoms of strangulation came on; and that she took, on that day, a draught of castor oil and turpentine, and had three injections, but without producing more than a trifling discharge from her bowels. Immediately after admission, several attempts at reduction were made, but in vain. She was then ordered a turpentine injection, administered by the tube, which brought away some feculent matter, but gave no relief. The tube could not be introduced higher than five inches. It was now resolved to perform the operation at seven o'clock, P. M. At that hour Mr. O'Reilly and Dr. O'Beirne met me in consultation. The latter introduced a thicker and better tube than that previously used into the rectum, to the height of nine or ten inches; a considerable explosion of flatus instantly took place, but without making any perceptible alteration either in the symptoms or the state of the hernial tumour. She was now exceedingly weak; her feet and hands were covered with a cold, clammy sweat; her pulse was 120, and scarcely to be felt; and the stercoraceous vomiting was incessant. The operation was again agreed upon, and proposed to her, but she obstinately refused to permit its performance. Under these circumstances, it was ordered that the abdomen should be fomented with warm water and turpentine; that the tube should be frequently introduced, and enemata of castor oil, Epsom salts, and warm water as frequently thrown up. After the second enema, some fecal matter came away. On introducing the tube to give the third, there was a great escape of flatus, on which

she expressed herself as being much relieved, and on examining the tumour it became evident that it contained omentum only. This occurred about midnight, and soon after she had a little sleep for the first time since her attack. The use of the tube and injections was afterwards continued every third or fourth hour, until about the middle of the following day, Sunday, by which time the bowels were freely emptied. During the greater part of that day and night she slept soundly. She recovered so rapidly as to be able to leave the hospital on the evening of the following Tuesday.”—(*Communicated by M. H. Stapleton, M.D., one of the Surgeons to the Charitable Infirmary, Jervis-street, &c.*)

Here are positive proofs of this mode of treatment having obviated the necessity of an operation in eleven out of sixteen cases of complete strangulation, where the usual and most efficacious means had previously been resorted to in vain. In this number the case of traumatic abdominal hernia is included, because, although the patient died of a wound in the stomach, the reduction of the protruded intestines, which had been found so difficult, was altogether owing to the introduction of the tube; thereby shewing the value of the plan in this as well as other species of hernia. But an inquiry into the circumstances attending the five unsuccessful cases, affords additional evidence in favour of the practice. Taking these cases in the order of their insertion, we find that Case No. III. could not be relieved by this means, because strangulation was not produced by flatus, but by solid and hardened excrement, completely blocking up the cavity of the strangulated bowel. In Case No. IV., the kind of tube employed did not admit of being introduced higher than five or six inches, and did not, of course, enter the colon: the practice, therefore, cannot be considered to have been submitted to a fair trial. In case No. VI., the uppermost part of the descending colon was so firmly constricted by a band of coagulable lymph, and so plugged up with solid feces on the cæcal side of this band, that even flatus could not pass down; consequently, there was, between the sigmoid flexure and the

strangulated portion of the ileum, an obstruction which effectually prevented the latter from being emptied by a tube passed into the former. In Case No. XI., the instrument was introduced but once, and as the patient recovered, and nothing remarkable was observed during the operation, it appears to me that the treatment was too hastily abandoned, and that, if it had been as perseveringly employed as it was by Mr. Stapleton in Case No. XVI., the operation might have been rendered unnecessary. In Case No. XII., the state of the parts, as observed during the operation, and after death, shewed that the patient's life could not be saved by human means. From this analysis of the five unsuccessful cases, it appears that two of them were not relievable either by operation or any other means; that one was only relievable by operation, and that in two others the practice was not submitted to a fair trial. It is only justice, therefore, to deduct three of these from the whole number, and not, perhaps, very unreasonable to add two to the list of those in which the treatment proved successful.

But let this amount of success be calculated or viewed how it may, it is manifestly such as ought to compel every conscientious and unprejudiced surgeon to give the practice a full and fair trial, before he decides on an operation which too frequently terminates fatally, and requires for its performance a degree of skill which many do not possess, and able assistance which may not be at hand. Such a trial causes very little loss of time, does not interfere with the employment of other remedies, is perfectly safe, and may be had recourse to after reasonable attempts at reduction have failed. For the reasons already stated, I have advised the treatment to be employed indiscriminately in all cases of strangulated intestinal herniæ, but there is an exception to this general rule, namely, the cases in which the hernial tumour is inelastic at all points, and evidently impacted with hardened excrement which cannot be forced into the bowels within the abomen. The cases in which its effects are most valuable and striking, are those of small, recent, and strangulated

enterocele, in which the constitutional disturbance is so severe and urgent, and the hernial tumour so tender, that the patient will not allow it to be touched, or cannot bear any attempts at the taxis. In cases where the intestine adheres to the sac, although it cannot effect reduction, it may either remove or relieve the symptoms of strangulation. But as far as my experience goes, it is as inapplicable in practice as it is in theory to cases of epiplocele. There is, however, a description of hernia to which I have not alluded, but in which it promises to be of nearly equal value. The description to which I refer includes not only congenital, vaginal, pudendal, and perineal herniæ, but also the obturator, ischiatic, mesenteric, and diaphragmatic, which, from not appearing externally, are ranged under the head of *internal* hernia. Should this plan succeed in the last-mentioned cases, it will do that which has been hitherto beyond the reach of all other means, except those of the most desperate and unpromising nature, such as cutting into the bowel at the supposed seat of obstruction.

Now that we see the remarkable effects produced by the discharge of flatus in cases of strangulated intestinal herniæ, let me ask a few questions. When, in such cases, we produce syncope, or a state bordering on it, either by copious venesection, the tobacco enema, the warm bath, or other means, the whole of the muscular system, and of course the muscular coats of the rectum, become proportionally relaxed, and the intestinal contents escape involuntarily, as they are known to do whenever a high degree of general weakness is produced. Is it in this way that these remedies act in such cases? We often find that, after all our means of reduction have failed and the operation has been decided on, the bowel suddenly starts up, of itself, into the abdomen. It is not uncommon for several surgeons to have failed in their attempts at the taxis, when another makes his appearance, and reduces the bowel at once, and with great facility. Is it by the accidental escape of a large quantity of flatus *per anum* that we are to account for such oc-

currences as these? The matter has not been attended to, but I have reason to believe that all these questions may be answered in the affirmative, and to suspect that patients are often wiser on the subject, than their professional attendants.

It is not merely, however, in obviating the necessity of operations, but in the after treatment of the latter, that the use of the tube is important. Every experienced surgeon knows, that, although the operation may be ever so well performed, and the ordinary means ever so judiciously employed, it occasionally happens, particularly when the operation has been too long delayed, that vomiting, tension and tenderness of the abdomen, and constipation will continue, and the patient die of peritoneal inflammation. Yet even in such an extreme case as this, it will be found that the timely introduction of the tube, and the use of enemata, will enable us to free the bowels, subdue the dangerous symptoms, and save the patient's life. That this is the fact, will be seen by the following case communicated to me about eight years ago, by my much regretted friend, the late Mr. Richard Gregory. This is the case to which I alluded, and which I promised to give, in discussing the elasticity of the the rings. It is as follows.

“ Some months ago, I found it necessary to perform an operation for strangulated inguinal hernia of the right side, on Mr. C., a farmer living at Ballymun, a few miles from Dublin. About ten hours after the operation, seeing that the very active means used to free the bowels had proved ineffectual, and finding the abdomen becoming more and more painful and distended, I passed an œsophagus tube up the rectum, to the height of seven or eight inches, and injected through it six pints of the fetid enema, with a glassful of turpentine. The bowels were very soon afterwards freely moved, and the patient recovered perfectly. Very recently, the hernia (also inguinal, intestinal, and on the right side) again became strangulated, and I was again compelled to operate on him. The same obstinate

constipation ensued, and the same means alone succeeded in overcoming it. Mr. C. is now living and well."

In point of fact, this is not a single but a double case, and exhibits two striking examples of the success which may be expected from the employment of a similar plan in similar cases. I have only to add, that, in the Richmond Surgical Hospital, whenever the bowels are not freed soon after operation, it has long been the custom to introduce the tube, and throw up enemata, and that this practice has been generally attended with the best results.

In my work on Defecation, I have described the different tubes and syringes which I had employed from time to time, in diseases of the intestinal canal ; but, from some strange oversight, have omitted to describe the most improved ; and as they are those which I continue to employ in all such cases, it is necessary to supply this serious defect. The gum elastic tube is sixteen inches in length ; considerably thicker throughout, and more bulbous at its upper extremity than that of the stomach pump ; at its lower extremity, it has a brass ferule, so made as to fit bayonet-wise into an aperture in a short pipe springing from a small brass syringe ; and to give it the necessary firmness, a delicate brass wire runs spirally through its interior. I employ this tube in new-born infants, as well as in adults. The syringe which I use is of brass, about seven inches long, and one inch in diameter, it is easily worked, very portable, and quite sufficient for all purposes connected with intestinal affections. In short, it is the small syringe belonging to the self-injecting apparatus of Mr. Weiss of London, to whose ingenuity I am also indebted for the necessary improvements in the gum elastic tube.

Having known that many intelligent and expert practitioners have failed in introducing the tube to the necessary height, and that others are fearful of using the degree of force sometimes necessary for that purpose, I may be permitted to offer a few simple instructions to the one, and some facts and reasoning to the

consideration of the other. As regards the difficulty of introducing the tube to the necessary height, it arises from the non-observance of the following rules. 1st. The instrument should be thrown into cold water until it becomes stiff, then be dried, made perfectly straight, and a few inches of its upper extremity well oiled. It is then to be introduced, the patient lying on the left side, and passed up inch by inch, and as nearly as possible in the course of the rectum. If obstruction be met with, it may be slightly withdrawn, and afterwards passed gently upwards. But if it cannot be introduced higher up without much force, attach to it the syringe, and dipping the point of the latter into the fluid to be injected, let an assistant give two or three rapid strokes of the piston, and so as to bring the full force of a strong and unbroken column of fluid to bear upon the point of resistance, and, while this is doing, let the surgeon urge the tube firmly upwards, and it will generally pass through the obstruction, as if through a narrow ring. 2ndly. With respect to the fear of using the moderate degree of force necessary to pass the tube into the colon, it is chiefly attributable to the fatal consequences which are known to have attended the unskilful management of rectum bougies, and the rude introduction of the metallic pipe of the common enema syringe. But the cases are quite different, and I shall now contrast them. The metallic pipe is some inches in length, and quite inflexible; the rectum bougie is short, thick, solid, and possesses little flexibility. With either of these, it is obvious that great injury may be inflicted on the bowel. The gum elastic instrument, on the contrary, is much longer than either, not so thick as the latter, tubular, bulbous at the upper part, flexible to the necessary degree, and, consequently, most unfitted for exerting any great or dangerous degree of force. Besides, its action must be that of dilating, not piercing, the sides or walls of the contracted rectum, for it is grasped, throughout four-fifths of its course, by the muscular coats of the intestine, and compelled to move in the axis of the bowel, and in no other direction. All these

points should be well weighed first, and then viewed in connexion with the well known fact, that it requires very great force to rupture living muscular fibre. But the argument which ought to have most weight with those who fear to use the necessary degree of force, is that which I have used in my work, and which increased experience enables me to repeat, namely, the fact, that I have neither met with, nor heard of, a single instance in which the introduction of the gum elastic tube has been followed by any evil or unpleasant consequence.

I have now to enter on a subject which I would willingly avoid, but the reader will soon see that it is forced upon me. It is this. When this mode of treating strangulated intestinal hernia was first made known, and for four years after, it was considered in all countries, as exclusively my own. I find, however, that my just claim to the merit of its discovery and introduction, is not only disputed, but actually transferred to another. But to whom does the reader suppose that it is assigned? It is assigned, *risum teneatis amici?* to a person whose country and whose name are both unknown, and upon grounds which will be found to be equally strange and objectionable. I shall now show that this is the fact. In the Bulletin General de Thérapeutique, for March, 1837, M. Lafargue inserted a paper entitled “Therapeutic Considerations on two new Means of reducing strangulated Herniæ;” and the two new means which he discussed were the application of cupping-glasses, as employed by Köhler, and the introduction of the gum elastic tube, as employed by me. In considering the merits of the latter, he commences by directing attention to a passage in a journal entitled “*La Clinique*,” for the 2nd July, 1829, and of which the following is a literal translation, viz. “We have found in a foreign journal the singular fact of a strangulated hernia being reduced in consequence of a vacuum attempted to be made by means of a syringe introduced into the rectum. This fact, which otherwise explains itself perfectly by physical laws, deserves to be verified by experiments.” After quoting this pas-

sage, M. Lafargue proceeds thus : “ This fact,” he says, “ was passed over unnoticed in France, perhaps it was to Dr. O’Beirne the origin of the thought of resorting to this proceeding ;”* (the introduction of the tube *per anum.*) This conjecture is so perfectly harmless, and hazarded so casually and with so much of studied reserve, that, of itself, it would scarcely call for a reply. But as it forms the basis of an unjust and distorted view of the matter, it is necessary that I should declare that the passage quoted by Mons. L. had not, and could not have had, any connexion whatever with my original ideas on the subject. I have not, and could not have, seen the passage in question, because there is not, as I have very recently ascertained, either a volume or a number of the journal containing it to be found in this city, in which I constantly resided for years before and after July, 1829. But even if I had seen it, I would have at once perceived that it was unworthy of serious notice, because it neither specified the country, the title, nor the number of the foreign Journal from which the “ singular fact” is professed to be taken; nor the country, the name, or the station of the individual who employed such a plan, and upon whose authority the truth of the fact mainly depends. In short, the only circumstances which led me to employ the treatment, were those which are so fully stated in my work on defecation, and in this article. But the matter cannot rest here, for my peculiar position makes it imperative upon me to show

* Voici ce qu’on lit dans le Journal *la Clinique*, numéro du 2 Juillet, 1829 : “ Nous avons trouvé dans un Journal étranger le fait singulier d’une hernie étranglée qui est rentrée par suite du vide qu’on a tenté de faire au moyen d’une seringue introduite dans le rectum. Ce fait, qui du reste s’explique parfaitement par les lois de la physique, mérite d’être vérifié par des expériences.”

Ce fait était passé inaperçu en France, peut-être a-t-il fait naître au Docteur O’Beirne l’idée de recourir à ce procédé.—*Bulletin Générale de Thérapeutique*, Mars, 1837. *Encyclographie des Sciences Médicales*, t. xvi. (2^{me} Série) Mars, 1837, i. p. 48, 49.—Art. *Considérations thérapeutiques sur deux Nouveaux Moyens de réduire les hernies abdominales étranglées*. Par G. V. Lafargue.

that M. Lafargue considered the treatment mentioned in the passage in question, to be both theoretically and practically different from mine. In describing one of Köhler's methods of effecting reduction, he states, that the vacuum produced by the cupping-glass, acts by drawing the protruded parts back and out of the ring; and consequently, he must have seen that it was in this way also, and not by emptying the intestine of its flatus, that the vacuum in the intestines acted when it was produced, as stated in the said passage, by the introduction of a syringe into the rectum. He knew that the said treatment was practically different from mine; first, in pumping flatus out of the intestines into a syringe, instead of allowing it to pass off through a tube into the open air: secondly, in using merely a syringe, which passed but two or three inches up the rectum, instead of a gum elastic tube introduced into the colon. Accordingly, we find that both the title and context of his paper show that he considered the practice as one of "the new means of reducing strangulated herniæ;" and that further than a mere conjecture, he never thought of questioning the justness of my claim either to the discovery or the introduction of the improvement, or to the peculiar views upon which it is founded. Yet, it is under such circumstances, and without any other means of supporting his opinions, that a gentleman of the highest character and rank in the profession, and one of the first authorities on the subject of hernia, has quietly dismissed my just claims to the practice, and handed them over to another.

The distinguished gentleman to whom I have just alluded is Mr. Lawrence, and I shall now quote his words on the subject. They are as follow: "The other method, of which M. Lafargue speaks in the paper above quoted, is that of procuring the discharge of air from the large intestine by means of a tube introduced at the anus. In the Journal entitled *La Clinique*, for July, 1829, it is mentioned that a strangulated hernia had been

reduced by withdrawing air from the large intestine, through a tube introduced, by means of a syringe. Dr. O'Beirne, Surgeon to the Richmond Surgical Hospital, Dublin, has employed this method with advantage, using the large gum elastic tube, which he introduces several inches into the canal, so as to allow the escape of air.—(*A Treatise on Ruptures*, p. 171, 5th Edit. Lond. 1838.)

Let me now analyse this extract *sententiatim*. In the first instance, M. Lafargue, who is merely a commentator on my practice, is mentioned so as to appear its introducer; in the second, an instance of the success of the treatment is quoted from a French journal, as having occurred nine years ago; and in the third, it is stated that I have employed “this method” with advantage. By this arrangement of the facts, the reader is plainly and unequivocally informed that instead of originating the practice, I have merely followed up that of another. But who is that other? Neither the editor of the journal called *La Clinique*, nor M. Lafargue, nor Mr. Lawrence, nor any one else to the present moment, can tell either his country, or his name, or who or what he is!! It is only certain that he is not a Frenchman, and it appears that he must not be an Irishman. In what journal or other work was the rival method originally published? That also remains unknown. Is there any theoretical or practical resemblance between the two methods? I have already shewn that they are, in all respects, quite different; but Mr. Lawrence has made them resemble each other, for he arms “the unknown” with *a tube, which he certainly never used*. If any one doubts the fact, he can convince himself of its truth by referring, first, to the second sentence in the above extract, and afterwards to the French note at p. 127, where he will find that no tube, nor any other instrument but a syringe, is stated to have been used. I do not accuse Mr. Lawrence of wilful misstatement, or of any intention or motive to do me an injury, for his high character will at all times protect him against such charges. I do accuse him,

however, of neglect in omitting to make sufficient inquiry into the facts, before he ventured to reject the just claims of even so humble an individual as myself. But he must now see that he has done me an injustice, the amount of which is proportional to the great celebrity and circulation of his work ; and he will feel, I am sure, that he owes it to himself, if not to me, to make all the reparation in his power.

BIBLIOGRAPHIC NOTICES.

Extracts from the Work of TESTA, Delle Malattie del Cuore, illustrating some unusual Symptoms of Pericarditis and Pleuritis. With Observations by WILLIAM STOKES, M.D., M.R.I.A., one of the Physicians to the Meath Hospital, &c.

THE work of Testa was published in Bologna in 1811, and is but little read in this country. The author's style is so extremely diffuse, and the date of the work so far behind the period of the recent and extensive discoveries on the subject, that it is very unlikely we shall ever see it in an English dress. It contains, however, some important and original observations, which bear every internal mark of accuracy.

Among these we may include Testa's account of carditis ; and in particular a class of cases in which the disease imitates affections of the throat. In his third volume, the fifth chapter is headed *Dei Pericarditici e Carditici Anginosi*, and contains those facts to which I desire to draw attention. After noticing the rare symptoms of maniacal excitement and destruction of the eye, as observed by Salius and Corvisart, Testa observes, "*Nessuno per altro, ch'io sappia, à fatto finora distinta menzione dei sintomi anginosi, li quali non sono sì uniscono ai segni proprii del cuore infiammato, ma bensì li nascondono quasi affatto sotto il solo apparecchio anginoso.*"—Vol. iii. p. 100.

In his first case, the symptoms of a most violent endo-pericarditis were those of fever, dysphagia, inability to open the mouth, and tumefaction of the neck. This case was communicated to him by an eminent surgeon, Signor F. Pelizo, of Spilemberg.

A peasant, aged upwards of sixty, was suddenly attacked with high fever, difficult deglutition, and the greatest difficulty in opening the mouth ; owing to the latter it was impossible to

observe the fauces. He was copiously bled, and the bleeding repeated the second day, without having any control on the progress of the symptoms; tepid bathing was applied to the lower extremities, and emollient applications to the neck, and cooling and purgative drinks administered. The fever was more severe on the third and fourth days; the respiration was most painful; pain in the fauces excessive. On the sixth and seventh days the strength of the pulse was somewhat diminished, but the difficult deglutition and pain in the throat remained. Eighth day, pulse still lower, when there suddenly appeared a remarkable swelling of the right parotid, which was dissipated the same day. The patient died on the tenth day. On examination after death the fauces did not present the slightest trace of inflammation. There issued merely from the trachea some serum. The pleura costalis and surface of the lung were only very slightly inflamed, but the mediastinum, diaphragm, the upper part of the stomach, and the convexity of the liver, which was very voluminous, were much inflamed. The thickened and hardened pericardium was filled with an abundant and foetid sanies. The substance of the heart may be said to have been completely disorganized, it was atrophied, and its external membrane totally destroyed, and its surface completely ulcerated. The parieties of the two ventricles were entirely covered with a gangrenous eschar, filled with a leathery substance like pseudo-membrane, which was also found in the aorta and coronary vessels. The branches of the pulmonary artery were much distended, and the vena cava and all the vessels in the vicinity of the heart were red and inflamed.

In less than a month after the above case was communicated to Testa, the following most remarkable example occurred in the Hospital of Bologna, under the care of Dr. Allessandrini: Maria Maccaferri, aged 63, had been healthy up to the age of 40, about which period she had an attack of angina, from which she perfectly recovered; nine years after she became subject to a long course of periodical fevers, varying from the tertian to the quartan type, and it was after having overcome these, chiefly by quinine, that she was attacked at intervals with tremors in the lower extremities. These tremors had returned the month before admission to the hospital with greater violence than before, so that she used to fall senseless on the ground. Some days after she was attacked with rigors, *followed by intense heat and most severe pain in the fauces, and the greatest difficulty in swallowing.* The third day these symptoms returned, attended by profuse sweats over the whole body. On the fourth she was conveyed to the hospital; the mental

functions were free, though she complained of weight and pain in the top of the head, and shewed a great disposition for sleep; her face was deep red; the fauces pained her severely; the parotids were swollen, and the tonsils and uvula were of a fiery red colour. Respiration was difficult, and similar to that of persons affected with angina; the voice low and feeble; there was no cough; but no substance, solid or liquid, could pass the œsophagus. The stomach was quiet; the pulse rather febrile; the heat of the skin little elevated; she passed no urine, and was often seized with rigors, affecting her whole person. *The diagnosis was made of angina pharyngea, with some laryngitis.* On the fifth day pain set in in the back; the strength was nearly exhausted. Before the sixth day she became comatose, and when roused from this sopor she was delirious; at such periods she swallowed with less difficulty; the evacuations from the bowels and bladder became more copious and involuntary; the pulse small and tremulous; the extremities cold; the face hippocratic. She died on the seventh day. On examination after death the lungs were found in many places attached to the pleura costalis and covered with pus. The pericardium was very much thickened, its internal surface unequal, and presenting small furrows and minute granulations, like the inner surface of an abscess. It contained a great quantity of whitish liquid, of a colour and consistence similar to pus. The heart was whitish and like boiled flesh; its external surface suppurated and granular; its tissue consistent; its fleshy fibres seemed relaxed, presenting a white and fatty substance, which, however, was not adeps, but rather a peculiar degeneration of the fleshy tissue. This latter appearance has been described before, amongst others, by M. Corvisart. The left ventricle alone preserved its texture, and the colour of its fibres; the cavities of the ventricles were full of coagula; the auricles and venous trunks were but little altered; the orifice of the aorta was surrounded by thick cartilage; and the parietes of the aorta thick, and somewhat dilated and radiated with red streaks through the whole of its arch.

The preceding cases are closely similar, and only differ as to the state of the pharynx. Yet we cannot but suppose that the inflamed condition in the last instance was an accidental complication, and that the dysphagia and other symptoms would have occurred had it never existed. In this case I may remark, that the *pain of the back*, and the *greater facility of swallowing pending the delirium*, are symptoms of great value. That the dysphagia was the result of a vital, rather than a mechanical effect, seems certain from the fact of its supervention at so early

a period in both cases. In the first example Testa states that the stomach was inflamed; but in the second, in which the dysphagia was still greater, he states that this viscus, with the spleen, was healthy, "*niente cangiati dallo stato loro naturale.*"

In the third case to which I shall refer, the stomach participated in the inflammatory state, and there was the combination of vomiting, with severe pain and smarting whenever deglutition was attempted. The fauces were not inflamed, nor was there any external tumour in their vicinity.

Edoardo Cocchi, a native of Bologna, aged 36, immoderately addicted to wine and spirituous liquors, had been often attacked by peripneumonia. In the month of April, 1806, having slept the greater part of the night quietly, he awoke suddenly with cold and trembling all over the body, followed by violent heat and deep pain under the xyphoid cartilage and in the right side of the thorax between the fourth and sixth ribs. Copious and continued vomiting, constant cough, acute pain in the head, and raving set in. In a few hours the vomiting ceased, but soon after returned, with severe smarting in the œsophagus whenever he attempted to swallow even a mouthful of water. The trachea was painful and seemed inflamed, so that respiration became painful. Neither in the internal nor external fauces was there any observable alteration. He remained five days in the hands of a surgeon, who bled him plentifully, and administered to him a great quantity of oil by the mouth; at the end of this time the vomiting had ceased, but there was persistence of the fever, cough, difficulty of breathing and of swallowing; incessant pain of the head, with occasional delirium and intolerable heat in the thorax, which extended from below the xyphoid cartilage to the fauces, accompanied by evident loss of strength. The surgeon then thought fit to send him to the hospital, where Dr. Rizzardi, of Bologna, collected the above history from the friends of the patient. On admission he presented the following signs: face pale; eyes watery and turbid; restlessness, caused by headach; great thirst; severe cough; the sputa mixed with yellow and red streaks; the breathing short; he spoke with pain, and had the greatest difficulty in swallowing. The tongue was moist and white; the colour of the skin natural; the pulse small, quick, and unequal, and occasionally intermitting; he was constantly uncovering his chest, being unable to bear even light bed-clothes; he was getting up in the bed every moment, then suddenly returning to the recumbent posture, and again he would wish to get on the floor. During the night he was greatly terrified by phantoms and

spectres. These symptoms persisted for thirty hours from the time of his admission into hospital, when he died wretchedly, bathed in a universal cold sweat.

On examination, the lungs were found in many places adhering to the costal pleura, and covered on their posterior surface by a false membrane, and appeared in some points suppurated, but they were not swollen, livid, and hard, on the contrary, they were soft and yielding, and somewhat pushed upwards by the abdominal viscera. The thorax, while the subject lived, was observed to have the form corresponding to this condition.

Though nothing remarkable was seen in the tissue of the pericardium, its sac contained a great quantity of thin sanies; the heart was somewhat hypertrophied and ulcerated on its surface. The tissue of the vessels was reddish and interspersed with minute red streaks. The right auricle was much dilated, and also the pulmonary artery, which very much exceeded the aorta in calibre. The diaphragm was inflamed, and so was the liver, the latter was very large and attached to the diaphragm; the internal and external surfaces of the stomach were highly coloured, and in like manner were all the intestines.

In commenting on this case, Testa inquires, whether the vomiting and dysphagia may not have proceeded from the gastritis, and also whether the heart had not become inflamed by sympathy with the digestive organs, and particularly the liver? That the vomiting was a result of the gastritis, there can be little doubt, but when we find dysphagia occurring in other cases, independent of gastric disease, it seems probable that it was in this instance produced (at least in part, by the inflammation of the heart. As to the second inquiry, I may observe, that without at all denying the influence of sympathy in inducing inflammation, (a fact which is familiar to every pathologist,) it seems more likely, that the diseases of the heart and stomach were coeval. On several occasions I have witnessed the diseases of gastritis, pericarditis, and pneumonia of the left lung coexisting. The patients had died of that peculiar form of typhoid fever, which follows an excessive debauch, and often sets in with symptoms of delirium tremens.

The following case presented all the appearances of angina, from disease of the heart.

Ant. Zambonini, a robust, well made, and athletic young man, aged thirty-two. He was in the habit of challenging other young men of his age to feats of strength, and was always successful. His principal feat was in ringing the heaviest bell in the city. On one occasion he excited the wonder of his com-

panions, by the length of time he continued to ring the great bell in the Cathedral of Bologna. Some days after he incautiously exposed himself to the cold and damp air in the beginning of the month of March. He was suddenly attacked with prostration and weariness; pain in the head, the frontal sinus, jaws and ears; his cheeks became like flame; eyes watery; high fever had set in. He remained ill about four weeks before he entered the hospital. The fever continued, with severe cough and difficulty of respiration, and pain in the fauces. In three days he complained of a severe burning pain deep in the chest; the cough and respiration were most troublesome, and the sputa slightly spotted with blood. About thirty-four days from the commencement of the disease, he complained of pain in the left arm, he was unable to raise it; he was then seized with smart rigors, followed by intense heat; and soon after by total loss of strength; soft irregular pulse, and involuntary motions of the lower extremities. Subsultus tendinum, burning sensations in the chest, and coma succeeded; and he died on the fortieth day. The extremities of the last ribs at the right side, were ossified; those at the left remained soft and cartilaginous; there were strong attachments of the lungs to the pleura, the former were covered with a dense thick pseudo-membrane, in which ramified many blood-vessels. The right lung floated in a straw-coloured serum; the ramifications of the trachea were hardened and resisting; their external and internal surface injected, they were full of white tenacious mucus. The pericardium was distended and filled with water; the heart was very large, red, fleshy, and compact, its base fatty. The right side of the heart (the anterior side) was filled with dense coagula, passing into the cavity of the pulmonary artery, which latter was of much larger calibre than the aorta. On emptying the left ventricle of coagula, which were much darker than those of the right, a large ulcerated surface was seen on the septum and immediately beneath the origin of the aorta, extending to the margin of the semilunar valves, one of which was quite corroded, and a fungous excrescence from the middle of the ulcer nearly closed the aortic opening. A small sinus was observed which allowed a probe to pass freely from the left into the right ventricle. The aorta was of the natural diameter, but was streaked with numerous red vessels; the diameter of the azygos was in many places double its ordinary size. This ulcer may have had its origin in some of those violent exertions to which this patient was addicted; perhaps from the forced return of blood into the left ventricle under the great contraction of all the muscles.

I have observed dysphagia as a symptom in thoracic inflammations, and its accompanying phenomena seemed to prove that it was less the result of any mechanical condition, such as pressure on the tube from effusion, than of some excited irritability either of the tube or of parts immediately in contact with it.

A woman, aged upwards of 60, of an extremely spare habit, was attacked with symptoms of acute lumbago after exposure to a draught of cold air. She remained for three or four days without paying attention to the symptoms, when the pain suddenly left the loins, and ascended to the inter-scapular region. When I saw her, her breathing was hurried; the pulse small and wiry; and she complained of an extraordinary sensation whenever she attempted to swallow. As the mouthful of food or drink passed down, a few inches below the pharynx, it excited a feeling of tearing or burning, through the remainder of the passage, which immediately subsided on the ingesta reaching the stomach. There was no regurgitation, but her sufferings from this symptom were extreme.

On examination, I found the lower portion of the left side sounding dull on percussion, with well marked egophonia at the root of the lung, and extending laterally for two or three inches. The action of the heart was rapid, but not irregular, nor were any of the direct signs of pericarditis present.

On the next day the heart was evidently displaced, and pulsated strongly under and to the right of the sternum, while it was scarcely perceptible in its natural situation. In the course of this case, the action of the heart became very irregular, but no other symptom of heart disease was manifested. After several relapses of the pleuritic symptoms, the effusion was absorbed; but on each exacerbation, the dysphagia became greatly aggravated, and was always relieved by the application of leeches over the affected portion of the left side. Since her recovery from the pleuritis, the irregularity of the heart has continued.

In two cases of pneumonia, I have observed symptoms somewhat allied to those described by Testa; I have seen a remarkable aphonia, without any other sign of laryngeal disease, set in, and subside with an extensive pneumonia of the left lung. The case was of a gentleman of full habit, who was attended by Dr. Graves and myself. The hepatization resolved with extreme slowness, but as soon as the side had recovered its sonoriety, the aphonia disappeared. This was a most insidious case.

In a young man attacked with pericarditis, the voice underwent a great variety of changes of tone, and was not restored

for several weeks, when all symptoms and signs of pericarditis had subsided. In this case, the liquid effusion was never very considerable. The phenomena were slight dulness, with various modifications of the rubbing sounds.

In the works which I possess on diseases of the heart, (with the exception of Testa,) I have not been able to find any notice of dysphagia as a symptom of inflammation of the pericardium or pleura. I have examined carefully the works of Senac, Corvisart, Bertin, Lænnec, Bouillaud, Hope, and Andral.* Testa alludes to the case of the wife of Polemarchus, recorded in the fifth book of the Epidemics, but I consider it as scarcely one in point. That of the courier in Morgagni, is more important, and I shall not apologize for introducing it.

“ Vir erat annorum amplius quadraginta, qui Foro Cornelii Bononiam identidem ventitabat pedes, res traditas huc illinc, et vicissim hinc illuc ferens. Is cum sæpe vel ab itinere calens, biberet, postremo præsertim tempore quo assidue sitiebat, *rheumate ad fauces gravi*, et febre correptus, in nosocomium admissus est. Mox ibi de faucibus non amplius conquestus, suum in ventre morbum omnem esse, dicebat; nulla tamen de re querebatur magis, quam de spinæ ad lumbos dolore, quo ea sibi media dissecari videbatur. Erant propterea qui intestinorum inflammatione laborare hominem, crederent: VALSALVA autem in thorace eam esse, suspicabatur. Erat autem pulsus debilis, humilisque; sed qui tamen ligatus, ut ajunt, videretur. Surgere, quasi abiturus, sæpe voluit. Per hæc intra tertium, an quartum ex quo in nosocomium venerat, diem confectus est. VENTER nihil habuit quod secundum naturam non esset. In Thorace autem ab altera potissimum parte humor stagnabat, in quo frusta natabant, quasi membranularum albissimarum; ut nihil magis referret, quam serum vaccinum, particulas retinens casei secundarii. Pleuræ vasa magis quam solent, rubebant, nec multo id tamen. *Pericardium vero fuit adeo distentum, ut vix compunctum, aquæ ejus qua erat plenissimum, tenue quasi filum ad non modicam altitudinem ejaculaverit. Cordis mucro plus æquo rubens, leviter inflammatus fuisse videbatur.*”—Lib. ii. *De Morbis Thoracis, Epist. Anat. Med. xvi. Art. 40.*

The foregoing facts all seem to prove that the symptom in question, however produced, is less a mechanical than a vital effect. It occurs in the earlier, sometimes in the very first

* In the Clinique Medicale, Maladies De Poitrine, a case of pericarditis is mentioned, in which the act of swallowing was immediately followed by a remarkable increase of dyspnœa. The false membranes covering the heart were more than an inch in thickness.

periods of the case, and at a time when but little distention of the pericardium has occurred. It may disappear in the more advanced periods of the case, and may be accompanied with analogous phenomena in parts, such as the larynx, placed out of the reach of pressure. Finally, when we consider the rarity of the phenomenon in hydro-pericardium, and in cases of empyema with the greatest eccentric displacement, we must, I think, adopt the above mentioned view of this curious and hitherto neglected symptom.

I shall conclude by stating, that I have lately observed in a case of sudden pleuritic and pericardial effusion, the singular phenomenon of *the thrusting upwards of the lung, so as to form a very large tumour above the clavicle*. The tumour had a puffy elastic feel, and the stethoscope detected an evident vesicular murmur all over its surface. The disease was subdued by active treatment, and in a few days the tumour disappeared. Here the left lung was suddenly compressed by the double effusion, and yet *no dysphagia* was observed.

WILLIAM STOKES.

On Orthopedics in France. By Professor J. F. DIEFFENBACH of Berlin. Condensed from the *Zeitschrift für die gesammte Medicin* of May, 1838.

THE progress which orthopedics have made in France during late years, has been very great, so that one may say, that the treatment of distortions and contractions after lithotrity, has been the favourite occupation of that nation. Only a few years back obliquity of the female form could hardly be considered as an object of surgical attention, because the admirable corset makers of Paris knew how to conceal all deformities of the spine in young girls.

Guerin and Bouvier are, amongst many other surgeons, the persons who have distinguished themselves most by their exertions in the relief of deformity at Paris. Both are highly informed, and endowed with much penetration and practical talent. Both these gentlemen are directors of the Orthopedic Institutions at the Village of Passy, in the neighbourhood of Paris. The establishment of the former, is the old Palace La Muette, a large edifice, surrounded by a beautiful park; that of the latter, a large and handsome private house, situated in a garden. In this place Milli had formerly an orthopedic institution, modelled after that of Heyne. These two establishments are equal in regard to the excellence of their system, and the attention paid both to body and soul. Religious in-

struction, mechanical treatment by appropriate apparatus and bandages, and properly selected gymnastic exercises, are employed alternately with judicious regularity. The difference of the two institutions are only external, and we know not whether we are more attracted in M. Guerin's by its splendour and greatness, which is, however, free from ostentation, or by the comfortable family life, in the more limited establishment of M. Bouvier.

Let not any one expect, that we are about to give an accurate description of the Parisian system of orthopedics, or to make known some extraordinary apparatus hitherto concealed, the construction of which we have comprehended in one glance of our eye, and which we are about to make known for the general good. I shall no more describe the known or unknown extensive beds, corsets, and twisting apparatus, which are as essential to the undertaking the cure of deformity, as is the form of the knife with which an operation is to be performed, to its success, than I should describe that knife, because every thing depends on the way in which we employ either.

What is most peculiar in the method of Guerin is, that when lateral curvature of the spine exists, the spinal column is not alone drawn to a perpendicular position, as is the case in the employment of all other extensive apparatuses, but by a prolonged extension, effected by the elongation of the bed, it is absolutely bowed over to the opposite side ; so that if the curvature be to the right, a curvature towards the left is produced and kept up ; if there be a serpentine curvature above to the right, lower down to the left, in the middle to the right, and below to the left, then all these curvatures are exactly counteracted and made to bend in the opposite direction on the extension bed. Although something similar to this has been effected by many German extension beds, furnished with belts and buckles which passround the body, still the bed of Guerin is peculiar in its construction and in its application. It is made of iron, and consists of many parts, which are not permanently united, but only screwed together temporarily. It has the appearance of three children's beds placed side by side, and over which a large man may be laid : or let any one picture to himself, the deck of a boat, let a deformed giant be laid upon this, and firmly bound ; when this is accomplished, let the boat be twisted right and left in the opposite direction to that in which the curvatures exist, and it will give a good idea of the extension bed of Guerin.

First, the person is buckled firmly to the bed, and then the portions in which the bed is divided are moved laterally. The

peculiarity of this bed does not alone consist in its divisions, but in its being made of thin iron rods, being very high, and being perfectly different and much more airy in appearance, than any other extension bed, and also in its having different springs and screws.

The spirit of order and propriety prevails in every part of M. Guerin's establishment, and although the great Monthyon prize has been adjudged to him, yet he does not consider his knowledge as at all perfect, but he still zealously studies to bring to perfection the branch of the profession which he has adopted: of this the preparations in comparative and human anatomy which his cabinet contains give sufficient proof. There is also a large collection of casts of distorted spines, with others of the same when rendered straight, and which are very instructive, and show in what extensive deformities a cure may be brought about.

Gymnastic exercises play a very remarkable part in the system of treatment; one large apartment is fitted up with the necessary machinery of all kinds, and it would astonish any one to see the lightness and activity with which the young girls perform the prescribed evolutions, and as the exercises proceed gradually from the lighter to the more difficult, no injury is ever caused by them.

The establishment of M. Bouvier, although not so large as that of M. Guerin, is admirably arranged. There we found beds something similar to the extension beds of Heyne, but of very simple structure, and differing from them in the extension springs, being bent in a half circle. There are twisting apparatuses of all kinds, by means of which, the most different exercises of the body can be performed with the greatest ease. Bouvier esteems these exercises of the greatest utility to persons who are crooked, because the body is supported as it is in swimming. The idea of causing young girls to go about on crutches, seems to me a most excellent one. When I first entered the garden, not being prepared for this sight, and perceiving a whole squadron of young French girls springing about with incredible velocity, I could not help involuntarily thinking of the kangaroos in Peacock's Island, and undoubtedly there was a very great similitude in the appearance of those practising this kind of exercise, to the short fore-limbs, and long hind ones of these animals. We are ordinarily only accustomed to see men go upon crutches, in order to relieve a lame leg, and their motions are always laborious, but the young ladies at M. Bouvier's, whose legs have nothing the matter with them, are genuine female virtuosos at crutch practice. As the shafts of

the crutches are very long, so those using them are enabled to make very great bounds, and so perfectly do they acquire the power of balancing themselves, that sometimes many of them are to be seen in groups chattering together, hanging by their arms alone on the crutches, with their feet half an ell from the ground.

This plan of extending the spinal column by the weight of the body alone, combined with the exercise of swinging and swaying it about in progression, is well worth imitation; but there are many cases in which it would be found very difficult to introduce the use of the crutches from the prejudices of mothers, who see nothing in the crutch, but the symbol of the cripple, and who consequently give the preference to machinery, which although injurious, can be concealed. M. Bouvier states, that such prejudices were very frequent, but that in despite of them, he succeeded in the application of crutches in the majority of the cases of curvature which he had treated.

M. Bouvier has also pursued his studies with unwearied zeal; and there is little doubt that his numerous examinations of the spinal column, both in man and in other animals, will yet afford a very rich harvest. I also saw here many interesting pathological preparations, in the completion of which M. Bouvier was yet partly engaged. There was a very large collection of plaster of Paris casts, each cast of deformity being placed in juxta position with another taken from the same person after treatment had been employed. Whilst we were looking at this medley of straight and crooked backs, M. Bouvier stopped suddenly before the cast of a curvature, and pointing to it said, "Voila une moule historique!" The story of it was as follows. An orthopedist of some skill at Paris, declared to the Institut of France, that he possessed the art of removing in a very short time, all curvatures of the spine. As this learned assembly always seek proofs, this surgeon presented to its notice, a grown up young girl, with a remarkable curvature of the spine, at the same time asserting, that he would reproduce this person in a short time before them perfectly cured. Commissioners were appointed as judges, and many casts were taken of the girl's back, of which Guerin and Bouvier both received specimens. In a very short time subsequently, the orthopedist appeared again at the Institut, and exhibited the girl who had been crooked, as a well grown young woman, without the slightest trace of obliquity. The Commissioners proved the identity of the girl, and all conversant in medicine were astonished at the miracle. The judges declared that this cure had been the quickest and the most perfect which they had ever seen. However the quick

witted Guerin harboured a doubt that there was some deception practised, and that the obliquity might have been simulated. He put this to the proof, by mingling many really deformed persons with others whom he had instructed to imitate spinal curvature, and no one was able to discriminate between them. Guerin, however, shewed, that in those having real curvatures, the muscles of the back were dislocated, but it was not so in those who simulated them. The zeal with which he unveiled this deceit, drew upon him a process at law, on the part of the pretender, who had gained the girl to his purpose for a sum of money, in order to make in short time a renowned name for himself as an orthopedist. Guerin, although in the right, nevertheless had a verdict given against him, and had to pay both costs and damages. Such was the end of an affair which took up much of the time of the French Academy.

I parted from M. Bouvier, who had devoted many hours to me, with the feeling that I had learned much that was new, but that which appeared to me most interesting was the going of deformed persons on crutches.

The Institut of France has rewarded the merits of Bouvier with the second prize of 6000 francs.

A short time ago M. Bouvier undertook the treatment of the poor who were distorted, some in the Hôpital des Enfants Malades, and some in the Hôtel Dieu; their number is yet trifling, and the patients are scattered about in different places. Great interest naturally attaches to this procedure of M. Bouvier, as in making orthopedics a portion of hospital practice, it will enable young physicians to learn them practically, which cannot be the case in private institutions, on account of the class of society to which the young female inmates belong. Besides the institutions we have mentioned, there has been another founded for deformities of all kinds, which has been placed under the care of MM. Bouvier and Duval. Here at appointed hours, is to be seen an immense crowd of distorted objects, principally children, who suffer from curvature of the spine or extremities from scrofula or rachitis. Club-foot is also treated. The greater number of the poor receive bandages and apparatus gratuitously. Bouvier treats the general deformities and Duval the club-foot. The former treats those cases of curvature which are not received into the house by fixing them in the girdle of Hossard, which has a spring passing diagonally over the back, secured to a bolt which passes round the shoulder and axilla, by means of which the shoulder which is lowest is elevated, and the upper part of the trunk forced over to the opposite side. The internal treatment of the de-

formed, particularly of children who are still suffering from rachitis or scrofula, appears to be somewhat neglected, and mechanical means appear to be too much trusted to.

The treatment of club-foot has also, during the last year, made great progress in Paris, and the useful discovery of Stromeyer of the means of curing this disease by cutting the tendon Achillis, and rational treatment afterwards, has been fully introduced. Before this period Guérin made use of machinery alone, then the plan which I introduced of pouring fluid plaster of Paris around the foot, so as to fix it in a proper position, but the invention of Stromeyer has been the most successful.

As the rest of this paper refers to club-foot and the operations for its relief, I have thought proper to omit it, as there is little in it different from the practice pursued in these countries since the publication of Stromeyer appeared.

S. L. L. B.

A Treatise on English Bronchocele, with a few Remarks on the Use of Iodine and its Compounds. By JAMES INGLIS, M.D., Physician to the Ripon Public Dispensary. pp. 95.

“WHEN I first entered on my duties at Ripon, as physician to the Public Dispensary,” says Dr. Inglis in his preface, “I was surprised to find so many goitrous cases on the records of that institution; further inquiry however, and personal observation, persuaded me, that this disease is as much endemic in some of the districts of Yorkshire, and other English counties, as in Geneva or any of the Alpine valleys.”—“I thought, therefore, that a pamphlet, written in some measure as well for the people as the profession, might be of use in calling attention to a disease evidently on the increase; and which, if it prove not directly fatal, may become so indirectly, by inducing a state of the respiratory and circulating organs, incompatible with health.”

Though we differ from the author in the expectation of good from any medical treatise addressed to the public, we think that he has succeeded in giving to the profession a valuable summary of facts and opinions, both those which have been recorded by his predecessors, and those taken from records to which he has had special access.

From its varying frequency and endemic character, bronchocele has given rise to many interesting researches, but though an approximation has been made towards a discovery of its true causes, by considering the circumstances common to all

situations in which it prevails, and also by admitting the influence of constitutional predispositions ; still, a complete solution of the difficulty is a desideratum.

Dr. Inglis has furnished several data for estimating the frequency of the disease in different localities. Thus, out of 5054 patients seen at Bishopton Grange, there were 111 cases of bronchocele. During the years 1835, 1836, and 1837, there were 993 general patients at the Ripon Public Dispensary, and of these nineteen were cases of goitre. From the year 1810 to 1835, inclusive, 62,228 patients were received at the Halifax General Dispensary, of whom 242 were bronchocelalous. From 1831 to 1838, there are 4139 cases on the records of the Pontefract Dispensary, of which sixty were goitres. It likewise prevails extensively at Knaresborough, Mickley, Borobridge, and Bedale.—pp. 36-42.

For the purpose of exhibiting the difference in the proportionate frequency of the disease, Dr. Inglis has given an extract from the records of the London Public Dispensary, Lincoln's Inn, from which it would appear, that out of about 40,000 cases, only nine cases of goitre occurred.—p. 47.

In taking hospital or dispensary registers as our guides in estimating the frequency of the occurrence of this disgusting deformity, we should always bear in mind, that they will fall below the real proportion, inasmuch as many labouring under the disease never apply for relief. It may exist for years without any inconvenience, and the sense of personal deformity is lessened by participation.

“ Of this any one may satisfy himself on a market day in Ripon, when by observing, he will be surprized to find so many girls with the disease, while so few apply for relief.”—p. 33.

As to the *causes*, whether constitutional, exciting, or proximate, they are very interesting, though somewhat obscure. In England, it would appear, that a vast proportion of the cases are females.

“ Prosser states, that in a village in Derbyshire, there were fifty girls labouring under the disease, and that he had never known a single instance of it, either in man or boy. At the Hampshire County Hospital, Dr. Crawford informs us, that during ten years there were admitted forty-nine cases of bronchocele, and that out of them, only one was a male ; i. e. nearly two per cent. On the authority of Dr. Forbes, we find, that at the Chichester Dispensary and Infirmary, the number of cases admitted within nine years, was seventy, out of which two were males ; the per centage being thus about three. At Bishopton Grange near Ripon, the number of

goitrous cases treated by Dr. Paley, within the last nine years, amounts to 111; of these, six only were males; being thus in the ratio of nearly five and a half per cent.”—p. 32.

Of the nineteen applying at the Ripon Dispensary, two were males. In the sixty cases occurring at the Pontefract Dispensary, there were two males to fifty-eight females.

In Switzerland and Germany the proportion of males is much greater.

As to the age at which it is most frequent, this may be considered as ascertained to be from fourteen (the time of puberty) to twenty.

“ We may infer,” says our author, “ that the first ten years of life are comparatively exempt from the disease, and that the second ten are most subject to it; as out of 111, only eleven appear during the first ten; sixty-three during the second, and twenty-four in the following; the fourth ten years present four cases, the succeeding, four; and from the age of fifty to sixty-two, only two cases are found.”—p. 37.

The experience of Dr. Copland confirms this conclusion; he says, (*Dictionary*, p. 269.) “ In a considerable number of cases which have come before me in females, I have never met with any before the period of commencing puberty, not even at the Infirmary for Children.”

From these facts, we can scarcely avoid the inference, that the organic development which takes place at puberty, has an intimate connexion with the production of bronchocele, and in females, this connexion is especially marked. Copland and Inglis agree in observing, that in most bronchocelatous females, the period of the first menstruation is delayed, or the due performance of that function interfered with or suspended. Again, in many cases noted by Dr. Inglis, an augmentation of the tumour was remarked on the occurrence of pregnancy.—p. 28.

After speaking on the sympathies of distant organs with the uterus, in explanation of this remarkable fact, Dr. Inglis observes :

“ May not then the thyroid gland sympathize with the uterus by some means or other? We know that the mammæ do, but the mammæ (at least parts of them) are of erectile tissue, and so is the thyroid. The thyroid may therefore be affected by the uterus, through the medium of the mammæ, and we found before that the thyroid was at this period (from the gradual obliteration of the thymus) in a state ready to receive any morbid impression, and what is more likely to give that than the suppression of a natural discharge of such importance as the catamenial? I stated before, that another corro-

borative fact is given to us by our patients, who affirm that during pregnancy there has been a sensible increase of the tumour, one quite distinct from that at the time of parturition ; which latter arises from an increased quantity of blood, i. e. congestion in the gland from suppressed respiration.”—p. 30.

We may add, as an additional evidence of sympathy between the genital system and the thyroid gland, that pain and sense of tension in the latter is not an unusual concomitant of menstruation, especially at its first establishment.

As to the effects of water impregnated with certain salts, it is impossible to reject it from the list of exciting causes. But it would also appear that water, changed by the presence of vegetable matter, may also give rise to the disease. Such is the opinion of Foderè, Gibbon, Clarke, and Johnson ; after noticing which, Dr. Inglis continues :

“ So far as I have yet heard, it would appear that lime, in some form or other, was present in the waters of all goitrous districts. This is tangible evidence, capable of proof from chemical analysis. But it would be difficult to say that some principle of vegetable origin was not also present.”—p. 23.

After objecting to the agency of the carbonate and sulphate of lime in giving rise to bronchocele, he adds :

“ I cannot say, however, that in all lime districts goitre prevails ; on the contrary, I know that it does not, in many places, where the blue vitrified mountain limestone is very abundant. Nevertheless, I think I am correct in supposing, that the presence of the *magnesian limestone* always predicts the co-existence of the disease. Take, for example, that ridge of magnesian limestone running from north to south through the centre of Yorkshire, and margining the shires of Derby and Nottingham ; all along that line we have goitre to a very great extent ; whereas, on our diverging to either side, the disease is found to diminish. The towns situate on the ridge are Nottingham, Alfreton, Chesterfield, Rotheram, Ackworth, Pontefract, Abberford, Weatherby, Knaresboro, Boro’bridge, and Ripon. After this, the magnesian limestone dips, then reappears in the county of Durham ; it continues its course almost due north from Darlington to South Shields, where at Tinmouth it meets the sea. In many of these towns I know goitre prevails ; in the others I should suppose it did, excepting when we approach within the influence of the sea, when the morbid action would be counteracted, for we found before, from Dr. Richardson’s statement, that the disease itself was removed by a sea voyage.”—p. 23.

On this part of the subject, we would especially direct the attention of Dr. Inglis to the researches of Mr. M’Clelland on

the occurrence of bronchocele in the Himalaya Mountains, as shewing that his views as to the connexion between certain calcareous strata, and the production of goitre, are not original. Mr. M'Clelland's observations were first published in Calcutta, in 1835, in a work on the geology of Amaon, and were reprinted in the eleventh volume of the Dublin Medical Journal, 1837, see pages 294, 340. Mr. M'Clelland's investigations on the relation of geological structure and the disease in question, are by far the most extensive and important that have ever been made. He has proved from a minute examination of a district of upwards of a thousand square miles, that cretinism and bronchocele occur almost exclusively in connexion with the calcareous rocks, of the transition and alluvial classes. He has shewn from a vast number of observations, that the proportion of goitred individuals in the granite and gneiss countries was but one in 500, and without cretinism. In the mica slate, hornblende, and steatitic sandstone, no instance of either disease occurred. In the clay slate and transition slate there was no cretinism; while the proportion of goitres was for the first but one in 136, and for the second, one in 240. *In the calcareous districts, one-third of the inhabitants had goitre, and one out of every thirty-two was a cretin.* Mr. M'Clelland states, that water is the medium by which the noxious principle is conveyed into the system, but an extensive series of analyses lead him to conclude, that he has not yet detected the ingredient to which its effects can be directly ascribed.

We feel convinced that Dr. Inglis will take these observations in good part, and we trust that he will continue his researches on this most interesting subject.

There is no question that the disease is hereditary; abundant evidence of this is to be found in the works of Clarke, Humboldt, Larrey, &c.

As to the pathology of the disease, our author remarks:

“The more common and simple form of the disease is when the gland appears sarcomatous, often interspersed with cells or cysts of various sizes, filled with fluid more or less viscid.”—p. 5.

Hedenus of Dresden, indeed, has found it converted into cartilage, with portions of osseous matter; and Alibert relates a case where it became scirrhus.

“From these different states of the gland, the appearance presented externally may be that of a tumour, with a well defined margin, or the marginal appearance may be insensibly lost in the surrounding cellular tissue. To the touch it may be at one time hard

and nodulated ; at another, soft, flabby, and of equal consistence.”—p. 6.

In detailing the medical practice in bronchocele, the author observes :

“It were needless long to remain examining the worth of the by-gone treatments ; suffice it, that the increased local action caused by heat or cold, frictions with or without stimulating lotions, embrocations or ointments ; by setons, blisters, or caustics, by electricity or galvanism, has been in many cases successful, in many the reverse, and in some even fatal.”—p. 54.

Burned sponge was a very popular remedy, and was found to contain iodine (discovered by M. Courtois in 1811) as its active principle. Various reports have been made as to its success in bronchocele by Coindet, Prout, Straub, Lugol, Elliotson, Manson, &c., all agreeing upon its efficiency ; and upon their recommendation it has been generally introduced into practice.

Dr. Coindet’s formula for its exhibition was, iodine 3 i. to alcohol 3 i. ; to this Dr. Inglis objects, that if it

“Were used in any quantity, the result would be a deposition of pure iodine upon the mucous membrane of the stomach, on account of the affinity which exists between alcohol and water. Nor would the evil stop here. Iodine has a great affinity for hydrogen ; so that, whenever it comes in contact with other vegetable or animal matter, it decomposes it, taking its hydrogen to form hydriodic acid ; the mucous membrane of the stomach would, therefore, suffer, which dissection after death proves really to take place, by poisoning with iodine ; there being always found, as Orfila has shewn, ulcerations of the mucous membrane of the stomach and intestines.”—p. 65.

“On this account I prefer, even to the weakest tincture, an aqueous solution, either of the hydriodate of potassa, the hydriodate of iron, or of iodine, rendered more soluble by the presence of a salt, such as the nitrate of ammonia, the hydriodate of soda, or the hydriodate of potassa.”—Ib.

But of all the preparations, Dr. Inglis gives the preference to the ioduret of iron. His formula is as follows :

Rx Iodureti Ferri, 3 ss.
Hydriodatis Potassæ ʒ i.
Aquæ Distill. 3 iss. Solve.
Capiat gtts. xx. ter die in aquâ.

“It was stated formerly, that in our goitrous patients, we generally found some catamenial irregularity, more particularly amenorrhœa. Now I have found that when the tincture of cantharides, and of the muriate of iron failed to induce the natural secretion, the ioduret

of iron often succeeded ; its use, therefore, is peculiarly indicated in bronchocele.”—p. 67.

The same remedy may also be applied to the tumour.

“ Of all the compounds of iodine now in use for the preparation of ointments, the hydriodate of potassa, (ioduret of potassium,) the ioduret of lead, and the ioduret of mercury, are the principal. All of them may be used in the treatment of goitre, but an ointment of the hydriodate of potassa ʒss; iodine ℥i; axunge ʒi. will be found to answer every purpose. Should the bronchocele be of short duration, soft, and small, it is only necessary to use the solution of the ioduret of iron internally ; but on the contrary, if the tumour be of long standing, hard, and large, then we may employ the ointment, of which, a portion about the size of a nut is to be rubbed into the enlarged gland twice or thrice a day.

“ Should the indurated bronchocele resist this treatment, I have seen decided benefit arise from the application of half a dozen leeches to it, at intervals of four or five days ; then administer the solution, and after the wounds caused by the leeches have healed, apply the ointment. Should still no abatement in the size of the tumour take place, at the end of three weeks, stop the remedies for a short time, and put on a blister ; when in a fortnight the medicines must be resumed.”—p. 69.

Thus cursorily (as our limits forbid much detail) have we noticed the principal points of this interesting essay. Some additional care might advantageously be bestowed upon the composition, and a future edition would be increased in value by a statistical table shewing the results of different plans of treatment.

The author has showed great industry and zeal in collecting facts, and we trust that the sale of his book will be commensurate with its merits, which we have no hesitation in saying, are very considerable.

The Visitor's Companion to the Botanic Garden, Glasnevin, comprehending a General Outline of the Principles of Botanical Science, with Illustrations and Popular Notices of various Objects of Interest in the Garden. By NINIAN NIVEN, Superintendent of the Royal Dublin Society's Botanic Garden, &c. &c.

The author's object is clearly explained in the following passages of the Preface :

“ For the purpose, if possible, of adding to the pleasure and enjoyment of those who visit the Royal Dublin Society's Botanic Garden

at Glasnevin, it has been conceived, that something in the form of the present work might be appreciated by the public.

“ It is with this object in view, that the “ COMPANION TO THE BOTANIC GARDEN ” is respectfully introduced to their notice.

“ Such a work, perhaps, will appear the more desirable, when it is recollected to what an extent the visitors to this delightful resort are annually increasing : having, during the course of the last four years, advanced from seven, to upwards of twenty thousand. So also, in like manner, has increased the attendance upon the Botanical Lectures, delivered in the Garden, by Doctor SAMUEL LITTON, whose learning, and affability of manners, are so well known, and so justly appreciated. Seeing, therefore, the impossibility of personal attention being bestowed upon such numbers, and feeling how important to the visitor, the stranger, or the botanical student, must be proper direction and guidance in such an extensive Garden, I am induced to present the following pages. At the same time venturing to hope, that the work may not be without its use in such gardens generally, judging from the natural similarity of their various productions and arrangements.”

“ It may, perhaps, be considered sufficient for me to state, without further reference in the following pages, that in the preparation of this little volume I have chiefly consulted the following excellent works, namely: ‘ Phillips’ History of Cultivated Vegetables,’ ‘ Stephenson and Churchill’s Medical Botany,’ Dr. Lindley’s admirable ‘ Introduction to the Natural System,’ Mr. Loudon’s various works, but especially his “ Arboretum et Fruticetum Britannicum,” (a work, I need scarcely say, that ought to be in the hands of every lover of trees, or of rural improvement,) and a few other modern works on botany and horticulture.

“ In an interesting book, published since the present work went to press, (D’Alton’s History of the County Dublin,) it is stated, in reference to our present annual expenditure, that, including all salaries, it amounts to from £1,500 to £2,000 ; whereas, *after including all salaries*, it only amounts to £1,020 per annum : a sum which, compared with the extent of the Garden, and the various objects it includes, I may be allowed to state, is inadequate.”

This guide will be of the greatest use to both occasional and habitual visitors of the charming Garden at Glasnevin, and will, we have no doubt, exert a most beneficial influence in encouraging the study of Botany in our Metropolis. The volume contains 179 pages, besides a plan of the Garden, and a tabular view of the principles of natural arrangement.

A Treatise on Inflammation. By JAMES MACARTNEY, M.D.,
F. R. S.

DOCTOR MACARTNEY'S *Treatise* is one of the most important which has issued from the Irish* press for many years. Doctor Macartney is peculiarly original on the subject of inflammation viewed as a restorative process. We shall let him state the question himself.

“ Mr. Hunter in his inestimable work on inflammation, when describing the union by what is called the first intention, seemed aware of the possibility of wounds healing without any inflammatory action ; he says the union in such cases, is without pain or constitutional disturbance, ‘ proceeds as if nothing had happened.’ In another place he says, ‘ There is only a feeling of tenderness in the part, and that is entirely from the injury done, and not from the operation of union.’ In treating of the same subject, he further says, that inflammation comes on as a necessary consequence of ‘ parts being too weak to unite by the first intention, or not having the power and disposition to heal.’ It is true, there are many other passages in Mr. Hunter’s work, which are in favour of the doctrine of inflammation, being within certain limits a sanative action, and the terms which he unhappily employed of *adhesive*, *suppurative*, and *ulcerative inflammations*, have misled those who have not studied and compared the different parts of his book with sufficient care and attention. The consequence has therefore been, that Mr. Hunter has been quoted as an authority for opinions, directly at variance with those principles of physiology, which it was the great object of his life to establish. If he had lived long enough to revise and republish his *Treatise on Inflammation*, he would, doubtless, have divested it of many of its errors and contradictions, and in making the different parts consistent with each other, he would have been compelled to deny the necessity of the process of inflammation to the healing of any wound. His treatise, however, notwithstanding the obscurity which attends many of his deductions from the facts he observed, and the unsuitability of many of the terms employed, is a rich and deep mine of pathological science, which we cannot hope to see rivalled by the labours of any future writer.

“ I have said that the received opinion of the present day, both in these countries and on the continent, is that some degree of inflammation is required, even for the union of the simplest wound. The practice of British surgeons is often the opposite, to what such

* Though London appears in the title page, the work was printed in Dublin ; we cannot account for this *error loci*.

an opinion would dictate ; but most of the Continental surgeons act in conformity with it, as they will not suffer any wound to heal, until it has gone through the processes of inflammation, suppuration, and granulation. The doctrine has been avowed by Sir Astley Cooper in the strongest language, in his *Lectures on Surgery*, published by Mr. Tyrrell. The passage may be taken, as expressing the general opinion of the profession in this country on the question. He says: '*Inflammation is a restorative process ; no wound can be repaired without it ; even the little puncture made by the lancet in bleeding, would inevitably destroy life, if this salutary process did not prevent it.*'

" I am well pleased, that the doctrine has thus been so clearly and unequivocally asserted, that no doubt can exist respecting the meaning of the author. It is also fortunate, that Sir Astley has selected the wound made in venesection as his example ; as I shall afterwards have occasion to quote the healing of this wound, as one of the strongest and most familiar instances of union being effected, without the slightest inflammation. The public will now have the opportunity of drawing their own conclusions, with respect to two opposite theories, founded on the same facts ; for, I presume there can be no difference of opinion, with regard to the actual phenomena, which attend the healing of the wound after bleeding with the lancet."—pp. 7-9.

We have many years considered that Dr. Macartney is right, and consequently that all other writers on surgery and physiology are wrong, on the question as above stated. It is unnecessary to point out the great practical results which must flow, and have already flowed, from the correction of errors so closely bearing on the treatment of wounds and injuries, nor will our space permit us to cite, from Doctor Macartney's *Treatise*, the numerous facts and experiments from which he draws a triumphant refutation of the opinions universally taught in England ; our author uses a style clear, condensed, and unadorned, to a degree which defies attempt at abridgment or analysis, and consequently we must content ourselves by extracting the whole of one of his chapters, while we refer the reader to the work itself, containing 214 pages, replete with instruction and interest.

" *Of the Different Modes of Reparation.*—Surgeons have hitherto described only two modes of healing ; to one of which the name of *union by the first intention* has been given ; the other is by means of granulations. Further inquiry, however, will shew, that this division of the subject is imperfect.

" Re-union and re-organization are effected in four different ways, which may be designated in the following manner :

" *First*, immediate union without any intervening substance such as blood or lymph.

“*Second*, the union by the medium of coagulable lymph, or a clot of blood.

“*Third*, re-organization without any medium of lymph or granulations, the cavity of the wound becoming obliterated by a natural process of growth.

“*Fourth*, the reparation by means of a new, vascular, and organized substance, called granulations.

“To the first of these modes of cure, I should wish to give the name of *immediate*. The second may be called the *mediate by lymph or blood*. The third, being compounded of different actions, I find a difficulty in distinguishing it by a single name. It might be denominated the *approximating* or the *modelling* process of reparation, or that by a *natural growth*. The fourth mode of union should be termed *mediate by granulation*.

“The three first mentioned modes of restoration, are quite incompatible with the presence of inflammation; a slight degree of which may, however, exist with the fourth. Not that I admit the growth of granulations to be an inflammatory process in itself. It ought rather to be viewed as the mode of reparation, adopted under the unfavourable circumstances of irritation, or a degree of inflammation being still continued, and proves that parts previously in a healthy state, are disposed to heal in despite of many impediments thrown in their way.

“The circumstances under which immediate union is effected, are the cases of incised wounds, that admit of being, with safety and propriety, closely and immediately bound up. The blood, if any be shed on the surfaces of the wound, is thus pressed out, and the divided blood-vessels and nerves are brought into perfect contact, and union may take place in a few hours; and as no intermediate substance exists in a wound so healed, no mark or cicatrix is left behind.

“We have familiar examples of this mode of healing, in slight cuts received on the fingers, which after being bound up, if no inflammation be induced, perfectly heal without the individual having any unpleasant sensation in the part, after the moment of the infliction of the wound. A case has been lately communicated to me of a considerable cut of the hand having been cured by this mode of direct union, without any sensation of pain, and in the short space of four or five hours.

“The incision made in venesection, if kept perfectly at rest, although it engages an important tissue,—the venous, may be healed with scarcely any intervening lymph, and with no consciousness on the part of the patient of having received a wound, except at the very instant when the puncture is made. The operation of bleeding leaves a line of white cicatrix, because it would not be justifiable to apply in this case the same degree of pressure, as in the small cuts of the fingers, for the purpose of retaining the edges of the wound in close contact. Every one knows that if the puncture of venesection

tion be provoked, to inflame, or in common language to fester, that its progress towards healing is interrupted; yet, this very wound has been quoted by Sir Astley Cooper, as an example of one requiring the aid of inflammation for its reparation. Those who contend for the necessity of inflammation, in all cases, for the reparation of injury, cannot refuse to admit, that in the instance I have just mentioned, none of the phenomena ascribed to inflammation exist; and that whenever the surgeon intends to accomplish union by the first intention, his success will depend on his being able to keep the parts in so easy and tranquil a state, that none of the phenomena of inflammation make their appearance. If there be any degree of inflammation, in which there are no heat, redness, tumour, pain, or disturbed vascular action, it ought to be clearly distinguished from that kind, which is attended with these phenomena; and then, we should have two sorts of inflammation; the one with phenomena, the other without, which, if we chose to disregard the logical contradiction involved in such an admission, would amount to the same practical result, as if on one occasion inflammation did exist, and on others did not.

“The union of parts with the medium of lymph or blood takes place in wounds, which either cannot, from the extent or shape of their surfaces, be brought into perfectly close contact, or where the parts will not sustain much pressure, without the danger of inducing inflammation. The lymph which issues from the adjacent surfaces, in the first instance, glues them together, and in a few days is found to have acquired some vascularity, and an imperfect degree of organization; after which, an external restraint for keeping the divided surfaces in contact becomes unnecessary.

“Mr. Hunter assumed, that the vessels arose in the lymph, and subsequently established their connexion with the vessels of the part, because, he observed, that vessels began to form in the membrane of the incubated egg, before they existed in the foetal chick. There is a great difference, however, between the original formation of vessels, and the acquisition of vascularity by lymph, deposited in contact with surfaces that are already organized; and it is more difficult to imagine, that vessels should commence in a clot of lymph or blood, than that they should be extended to it from the adjoining surfaces. It is also impossible to conceive that the thin layers of lymph which unite serous membrane, or the effusion which consolidates cellular structure, do not obtain their vascularity from the adjoining parts. Further, I have seen vessels passing for a short way into a clot of blood, covering the surface of an ulcer, when the coagulum possessed no vascularity of its own. I have also succeeded in forcing injection into the coagula formed in the cavities of the heart after death, which injection presented the appearance of red elongated lines. I am therefore induced to dissent from the opinion of Mr. Hunter on this subject.

“Every surgeon knows, that if wounds united by means of lymph

be provoked to inflame by motion, friction, too much constraint, or any other cause of disturbance, the tender and half organized medium is destroyed, and the chance of union by the first intention is lost. We cannot, therefore, with any consistency, describe the adhesive process as an inflammatory one, when we perceive that it is interfered with by any sensible degree of inflammation.

“ When the uniting medium of lymph is fully organized, it possesses a considerable degree of vascularity, and can be rendered red by injection, as we observe in callus of bones, and the adhesions of serous and cellular membrane. For the same reason, the cicatrices of wounds appear red for some time.

“ Depositions of lymph, made for adhesion or reparation, may in the end acquire, as nearly as possible, the original structure of the neighbouring parts. Thus, when serous surfaces are conjoined, the uniting medium becomes thin and membranous, and elongated, in proportion as motion may be requisite. The callus of bones acquires the osseous structure, and the cicatrices which form on the external surface of the body bear an imperfect resemblance to the skin.

The effusion of coagulable lymph is always more abundant than is ultimately necessary for reparation; and hence it is gradually diminished by interstitial absorption, and thus the lips of a wound, which held between them a plate of lymph nearly one quarter of an inch in thickness, may come so close together as to exhibit, in some cases, little more ultimately than a thick white line of cicatrix. Usually, however, when much lymph has been employed in the union of parts, a thick, hard, and puckered cicatrix is formed and remains.

“ The mode of reparation by the modelling process, has never been described; because surgeons, heretofore, did not know that it was possible for open wounds to heal without inflammation, in the higher classes of animals. However, when healthy parts are injured, although it may be to the greatest extent, if placed under the most favourable circumstances for carrying on their natural actions, the process of reparation is nearly the same, even in the human subject, as that which I have described as belonging to the animals of a simple structure. The pain arising from the injury soon ceases. No tumefaction ensues, separating the edges of the wound, and its surfaces are not only disposed to lie in contact, but even to approach each other so much, that they cannot be kept asunder by mechanic restraint; there is, therefore, no necessity for the effusion of lymph; and as there is no cavity to be filled up, granulations are not formed. The surfaces of the wound, although they come into contact, do not unite by vessels shooting across; they are smooth, red, and moistened with a fluid, which is probably serum, and present the appearance of one of the natural mucous surfaces of the body. If any parts have been killed by the injury, they are separated, by simply as much interstitial absorption as is sufficient to set them free. The wound is finally healed by the same means which determine the shape of the

natural parts of the body. It gradually diminishes in extent until it is obliterated ; or it may be cicatrized before the surfaces are abolished, after which the same process of natural growth goes on, until no part of the original wound is left. The cicatrix which succeeds the cure of injury by the modelling or growing process, is small, pliant, free from those callous adhesions to the parts underneath, and the morbid sensations that so often belong to those cicatrices, which have for their bases the deposits of lymph, or the new formed structures called granulations. When the modelling process or cure by natural growth goes on perfectly, there is no inflammation in the part, and the patients are so entirely free from all uneasy sensations, that I have known instances of their being ignorant of the real site and extent of the injury, until they had examined the part with their hand, or saw it in a looking glass.

“ It might be anticipated, that as this mode of reparation bears so strong a resemblance to the natural formation and development of parts, it is the slowest mode ; but this is of little account, when compared with its great advantages, in being unattended with pain, inflammation, and constitutional sympathy, and leaving behind it the best description of cicatrix. It constitutes the nearest approach in the higher classes of animals, to that regenerative power which is exhibited by some of the inferior tribes.

“ When parts have been removed by the process of ulceration, or kept asunder by some degree of inflammation, we find the vacancy will be filled up by the growth of granulations, instead of organized lymph. It would appear that the granulation structure, being endowed with more vascularity and a higher degree of organization, than can be acquired in a short time by effused lymph or blood, is the reason of its being formed for the purpose of reparation, when the parts are placed under unfavourable circumstances to accomplish a cure ; for we find, that if blood or lymph be shed on a common ulcer, by incisions of its surface, and perfectly enclosed so as to avoid all external irritation, and kept in an easy state of sensation, it unites with the surfaces of the ulcer, and acquires organization, as when the same substance is shed in common wounds.

“ The existence of granulations has been supposed to be necessary to fill up deficiencies ; this, however, is not the correct explanation, for we meet with very considerable vacancies filled by lymph, which is never converted into granulations ; as in cases when recent incised wounds are imperfectly closed, but nevertheless are healed by the first intention. The necessity of the granulative process seems entirely to arise from the parts being in that degree of excitement, which is not enough to prevent reparation altogether, but to permit it to be effected by a highly vascular medium. Thus, in simple fracture the union is by the medium of coagulated blood or lymph, although the quantity of new material required is often greater than in compound fracture, where granulations are employed. If the cavity of an opened joint inflame, it is filled with granulation ; but if inflammation

be presented after such an accident, its cavity may be obliterated by effused lymph. I recollect seeing a case of most extensive laceration of the leg, in which the deficiency produced by the injury was filled up by pale organized lymph, instead of granulations; the reason of which was, that so much blood had been lost, that no inflammatory action had been excited. And further, it is only in the beginning that granulations take the place of the natural structure; for the approximation of the edges of a wound or of an ulcer is accomplished by the interstitial absorption; and finally, wounds that are healed by the granulative process, exhibit no more remains of the new medium employed for union, than if lymph had been the substance employed. Granulations, therefore, exist for a special purpose, and that being effected, they cease to occupy the place of the original structure longer, or more than is necessary. The ultimate absorption of the granulations is something like the contracting or approximating action, which exists in open wounds that have never inflamed; and it does not take place until the inflammation has subsided in a common wound or ulcer.

“The organic structure of granulations is very peculiar; although easily destroyed by injury, or a high degree of inflammation, it is endowed with important vital properties.

“A deposition of lymph, after being united to an opposed surface, as in an ulcer, or in the interior of an abscess, may assume the granulation structure; but the usual manner in which it is formed, is by deposition and organization going on together, as in the growth of the natural parts of the body. In whatever mode granulations are produced, they are composed of a fine cellular membrane, into which blood-vessels proceed from the subjacent tissue. It is understood that these vessels have their branches directed to the surface of the ulcer, without much radiation or inter-communication; and perhaps, the little eminences on the surface, from which the name of granulations has been given to this structure, may be produced by the force of the circulation being unequally distributed, or directed from the small trunks underneath, on particular parts of the surface of the sore. Whether this explanation would satisfy us or not, it is the fact, that in proportion to the activity of the circulation the granulations are large and eminent; and when it is languid, they are minute and of a brown colour, as in the irritable ulcer; smooth, flat, and glossy, as in some old venereal ulcers; and in the indolent ulcer the surface is not granular, but depressed, hard, striated, and is nothing more than very imperfectly organized lymph.

“The surface of granulations is always covered by a delicate pellicle, somewhat similar to the almost invisible integument of the mucous membrane.

“I am not aware that any person has traced nerves into the substance of granulations; but no doubt can be entertained of their existence. The natural sensibility of granulations is very similar to that of the vascular, and delicately covered surfaces of the true skin;

a gentle touch to the surface of a healing sore is agreeable. The feeling in granular structure is liable to be augmented by inflammation. In various diseased states ulcers are affected with different kinds of pain. They are also excited by any species of mechanic injury, or external cause of irritation.

“ Besides the sensibility which granulations possess, in common with the external surfaces of the body, they enjoy also that of the mucous surfaces in an eminent degree. Like the stomach and intestines, they are capable of distinguishing the different qualities of substances placed in contact with them, independently of mechanic impression. Upon this species of sensibility of granulations, the effect of many applications to ulcers solely depends. One kind of ointment must produce the same mechanic effect as another : and the litharge plaster, whether it may be spread with too much heat or not, can make no difference in the qualities of its surface ; and yet we know that the greatest variety of effect may be produced on ulcers, by the composition alone of their dressings. It is the peculiar sensibility of granulations, which causes them to select the objects for absorption, in the manner of the mucous coat of the alimentary canal ; and as they resemble in their sensitive properties, both the skin and the internal surfaces, the condition of an ulcer is liable to be much influenced by the state of the constitution.”

The Life of Edward Jenner, M. D., F. R. S., with Illustrations of his Doctrines, and Selections from his Correspondence. By JOHN BARON, M. D., F. R. S., &c. &c.

IN reviewing the sources of the scientific distinctions of individuals—distinctions awarded by the unanimous voice of civilized man—it would appear, that as the tide of human knowledge flows onward, gathering strength and extent in its course, the probabilities of such honours being attained, will be every day less and less. Man has become familiarized with science, and the very number and facility of her paths may act in retarding the mind in its journey through any one of them, so as to surpass all competitors. But there are two kinds of distinction : one, which attends great talents successfully employed on the whole of one or more sciences ; and the other, that which is given in return for some single boon, itself so great, as to come home to every man, to compensate for all other labours, and to cover all possible deficiencies in the object of our admiration and gratitude. Such are the honours that the world awards to the memories of Galileo, of Bacon, of Newton, of Harvey, and of Lænnec. Such too

will it give to Jenner, the benefactor of mankind of every class and every clime.

The task of writing the history of this great man, or of his discovery, has been executed by his friend Dr. Baron ; and we hesitate not to say, in a manner which will satisfy posterity.

The work, besides its inherent interest, is a great addition to our medical literature. It is written in a manly, clear, and often eloquent style, and is in truth what we might expect from a life of Jenner by Baron ; of a great physician by a great physician ; of a discoverer by one himself a discoverer ; of a friend by a friend. Were we to make any exception, it would be, that Dr. Baron has been perhaps too minute in recording all the annoyances and vexations which Jenner suffered from ignorant opposition, or haughty neglect. The too frequent recurrence to these topics, leaves an unpleasant feeling on the reader's mind, and disfigures a landscape, which would be otherwise perfect. Certainly, much of the opposition which Jenner experienced, was most unfair and vexatious. The public seldom reasons well on any subject in the first instance. It seldom takes the trouble of rightly understanding a doctrine ; and on the other hand, reasons falsely as to its evidences and exceptions. Thus, because Jenner inoculated his own child with variola, after the failure of vaccine, he was represented as a cheat, or as having abandoned his opinions.

“ The facts were these : on the 14th of May, 1796, Jenner vaccinated his first patient, Phipps. On the 12th of April, 1798, he vaccinated his son Robert, together with several other children. It is particularly specified in his first publication that his son Robert “ *did not receive the infection.*” He was, therefore, as much liable to the influence of small-pox contagion as if he had never been vaccinated. Under these circumstances, while the infant was with his parents at Cheltenham, the late Mr. Cother of that place came into Jenner's house, and took the child in his arms, saying that he had just left a family labouring under small-pox. Jenner immediately exclaimed, ‘ Sir, you know not what you are doing. That child is not protected. He was vaccinated ; but the infection failed.’ Believing that the natural small-pox would certainly follow this exposure, he was greatly distressed and alarmed. He had no vaccine matter. He resolved, therefore, to adopt the next best expedient, and immediately had the child inoculated with small-pox virus, preferring the mitigation which that practice affords to the violence and danger which generally accompany the casual disease.

This simple occurrence, when related as it actually took place, so far from leading to the conclusions that were built upon it, did not afford the slightest ground for them. It was a clear case of professional duty ; and, under like circumstances, every medical man

would have been called on to act as Jenner did. He had no vaccine matter; his child was exposed to small-pox contagion; and what, therefore, did he do? Small-pox, in some shape, seemed inevitable; and he sought for that abatement of its virulence which inoculation is known to afford."

It was further declared, that Jenner put forward vaccination as a *certain preventative* against variola; and hence every case of small-pox following vaccination, was blazoned forth in the strongest colours. Each successive case was held to be a refutation of the entire doctrine, and a proof of Jenner's ignorance or deceit. Yet Jenner never declared that vaccine was an infallible preventative.

"Duly and efficiently performed," he observes, "*it will protect the constitution from subsequent attacks of small-pox as much as that disease itself will. I never expected that it could do more, and it will not I believe, do less.*"

Some excuse, however, must be made for the public for believing that the cow-pox was a complete preventive of variola, when we find that in the declaration of the London physicians in favour of cow-pox, this very doctrine is expressly stated. We subjoin part of this celebrated declaration.

"We the undersigned physicians and surgeons think it our duty to declare our opinion, that those persons who have had the cow-pox are perfectly secure from the infection of the small-pox, provided such infection does not exist in the system at the time of the inoculation for the cow-pox."

This, it must be allowed, was sufficiently strong, but when we find the names of thirty-eight practitioners attached to the document, and among them those of Baillie, Lettsom, Willan, Crichton, Denman, Cline, Cooper, and Abernethy, we cannot wonder at the public misapprehension.

But Jenner was exposed not only to misapprehension, but to the coarsest ridicule; in which, we regret to say, many of his own profession assisted. Of these, the most formidable was Dr. Mosely, from whose pamphlet now lying before us, we extract the following passages, as illustrative of the style of medical pamphleteering forty years ago.

"In the year 1798, the cow-pox inoculation mania seized the people of England *en masse*.

"It broke out in the month of April, like a symptomatic eruption of nature; the planet Mercury—the delusive author of 'vain and fond imaginations,' being then in the zodiacal sign of the Bull.

"It increased as the days lengthened; and at midsummer,

large societies of the medical profession, which was first attacked, were distempered to an intolerable degree.

“Men who before this epidemic raged in their veins, were not noted for any particular ill manners or extravagant behaviour, and went about their professional affairs in the ordinary way; now possessed of the Taurine spirit, became very alarming, and were not to be met at the bed-side or even in the streets without danger.

“Great events are foreboded; some pretend that a restive greasy heeled horse will kick down all the old gally-pots of Galen; others, that the people of England are becoming like the inhabitants of a wilderness beyond the land of Cathay, seen in 1333 by the rare Sir John Mandeville; who, he says, were ‘wild, with horns on their heads, very hideous, and speak not, but rout as swine.’

“The virtues of this charming distemper are said to be an amulet against the small-pox, that it is mild and innocent, and communicated with safety by inoculation.

“Wonderful things do certainly appear in all ages; the great Erasmus mentions a man, one Philario, an Italian, who in Holland was very much afflicted with worms. While the worms were in his body he spoke the Dutch language fluently; when his physician cured him of the disorder, he could not speak a word of that language. The Dutch worms and the Dutch language left Philario together.”

“Can any person,” he continues, after a page or two, “say what may be the consequences of introducing a bestial humour into the human frame after a long lapse of years?

“Who knows, besides, what ideas may arise in the course of time, from a brutal fever having exerted its incongruous impressions on the brain?

“Who knows also but that the human character may undergo strange mutations from *quadrupedan* sympathy, and that some modern Pasiphaë may rival the fables of old?”

As might be expected, Jenner’s life, from the publication of his inquiry, was scarcely a happy one. He had to undergo the greatest labour in attending to a vast correspondence. He had to bear alternately the shafts of ridicule, the malice of enemies, the excitement of controversy, the ignorance of friends, the ingratitude of his own country, and that insulting mixture of patronage and neglect; which all must suffer who come in contact with the great. Still the cause of vaccination advanced, and he lived to see it spreading into every quarter of the globe, and to receive the thanks and admiration of men of every country. Such was the reverence attached to his name, that a

certificate signed by him, answered as a passport for protection during the war; and on more than one occasion his intercession with Napoleon procured the discharge of English prisoners. We blush to record, that this noble example was not followed by our own Government, who in the case of a French officer, a prisoner of war, refused to Jenner the boon which our great and enlightened enemy had granted. It is true that England shewed her sense of gratitude by a parliamentary grant, but we do not believe that its amount was sufficient, or at all commensurate with the boon conferred by Jenner on Great Britain and the world. And we find that, with the exception of an honorary degree from Oxford, and the freedom of the cities of London, Dublin, Edinburgh, and Glasgow, he hardly received any honorary distinctions in his own country.* On the continents of Europe and America, the case was widely different. He died on the 26th of January, 1823. His friend, Sir Gilbert Blane, felt that Westminster Abbey was the fit place to receive his remains, and that a public funeral was demanded by justice. But the Government were not anxious about the matter. A misunderstanding arose, and the earthly remains of Jenner were committed to the grave in a place more consonant to his life and character—in the silent and peaceful isle of Berkely Church, where she who had been to him as “peace amid the billows” had long been laid.

The following is a specimen of good writing and sound sense.

“It is at once my duty,” says Dr. Baron, “while it yields me pleasure, to present the gratifying details which have just been closed, in connexion with the personal feelings and character of Jenner himself. Unhappily, his race was run before the full tide of gratulation, with which the discoverer of vaccination might now be hailed, had reached his ear. The reader, however, cannot fail to observe that, even from the outset, Jenner’s confidence was as firm as it was just and well-founded. But, after all, it is a marvellous subject; and even those who have watched its progress, who have anticipated and longed for the success that has attended it, cannot, when the accumulated evidence is brought fully to bear upon the lives and happiness of kindreds, and tongues, and nations, avoid wondering at the signal mercies with which, in these days, Providence has crowned the exertions of one of our fellow-creatures.

“Wonderful as have been the events of the last years, great as

* For the freedom of the City of Dublin, Dr. Jenner had to pay fees amounting to nearly five pounds !!

has been the advancement of human knowledge, and rapid as has been the progress in all the arts calculated to promote the comfort and convenience of life, the most remarkable phenomenon is, certainly, not that we have subdued the elements to our use—not that we can multiply at will the products of our ingenuity—not that we have brought mechanical agents to take the place of active and intelligent beings ; but that we have been enabled to stay the power of death—to keep him for a season from his victims—and to say that the day of grace and preparation has been lengthened.

“ In thus exulting in the benefits of vaccination, it will be seen that I cannot fall in with the lamentations of the whining economists who look upon an increase of population with an evil eye, and permit selfish and limited views of what is best for the well-being of the community to interfere with the richest blessings of Providence to man.”

We cannot conclude this brief notice, without adverting to the extreme ignorance of vaccination exhibited by many medical men. We know not a few who could not tell a genuine from a spurious vaccine vesicle, and who were ignorant of the entire subject, both as to its previous history, and the appearances and management of the disease. This ought not to be. We would compel every advanced medical student to attend at a cow-pock institution, where a proper system of instruction should be organized. In Dublin we have the best opportunities for such a purpose ; we have an institution which has always worked well, and has effected a more extensive system of vaccination in this country than is generally known ; and we need only mention the name of Dr. Labatt, as connected with the institution, to show how great would be the advantages derivable to the student from such a course of study.

The day when medicine was practised without instruction has passed by, the public now demand practical knowledge.

On this subject we have great pleasure in quoting from Dr. Labatt's Address to the Practitioners of Ireland, on the Cow-pock, published in 1805.

“ The frequent reports of cases in which the cow-pox has been supposed to fail in affording the promised security against small-pox, has tended very much to impede its general diffusion in Ireland. Since the introduction of vaccination into this country, having bestowed considerable attention upon it, and spared no pains in examining with impartiality these alleged failures, it affords me much pleasure to state, that on an accurate investigation, I almost invariably found them to originate either in ignorance, or absolute and wilful misrepresentation of facts, of which I could adduce some remarkable instances.

“ From the careless manner in which the new inoculation is performed by inexperienced practitioners, it is contrary to reason or common sense to expect, that adverse cases will not now and then occur. But if it has been proved in a hundred thousand instances, or more, that cow-pock inoculation is a certain preventative of small-pox, how can a few solitary cases of reputed failure stand in competition with such an established law? The inattention and ignorance of practitioners have often brought into discredit the most active and efficacious remedies; in no case has this been more remarkable than in the practice of vaccination, which some have presumed to commence without having ever seen the disease, or read a book upon the subject. That such people should be deceived as to the characteristics of the disease, can excite no surprise; and that their rash and inaccurate reports must lead to conclusions very prejudicial to the reputation of cow-pock.

“ Although this disease be slight and the inoculation of it very simple, yet a previous knowledge of the complaint is not the less necessary. Dr. O'Reilly, one of the most learned physicians of Prague, in a letter to Dr. De Carro of Vienna, observes, ‘ that a spirit of observation is much more necessary in the inoculation of cow-pock than in that of the small-pox.’ And Dr. De Carro, himself, as well as most other experienced practitioners, is of the same opinion. A correspondent of Mr. Ring, Mr. Erving, is fearful lest ‘ the practice being so very simple, and the operation and effects of the disease so innocent, it may descend into the hands of the ignorant, careless, and unwary, so as to defeat all the promised good effects, and leave the patient, by throwing him into a false state of security, in a more perilous situation from the variolous infection than he was at first. When we reflect that every person who is able to hold a lancet, thinks himself capable of inoculating with cow-pock, and that little attention appears to be paid to its investigation, we cannot but be astonished that more instances of failure have not happened.’ ”

W. S.

The Anatomy and Diseases of the Testis. By ROBERT VOGAN, A.B., L.R.C.S.I., pp. 354. 12mo. Dublin, Hodges and Smith, 1838.

WHEN first we saw the announcement of this treatise on the diseases of the testicle, we could not help feeling that after the numerous excellent works already published on the subject, particu-

larly the excellent work of Sir A. Cooper, a new one was scarcely called for, and we had considerable doubts as to its prospect of success. We trust that on the last head we may be deceived, and that as Mr. Vogan's book is really possessed of a certain kind of merit, it may not disappoint its author's expectation. The merit we speak of consists in its being a very good compilation from the works of Ramsden, Sir Astley Cooper, Dr. Titley, and Samuel Cooper, and as such we think will be found useful to the advanced student, near his examination, who wishes to make himself up on these different authorities, but has not time to read them all; or to the country practitioner, who has not the authors mentioned above within his reach. In the arrangement and exposition of the various diseases of the testis, Mr. Vogan has shewn considerable judgment.

A Practical Compendium of the Materia Medica, with numerous Formulæ adapted for the Treatment of the Diseases of Infancy and Childhood. By ALEXANDER URE, M.D., M.R.C.S. London, Schloss, 1838.

WE have examined this volume with great care, and have risen from its perusal with the highest respect for the judgment and information of the author; he has supplied a great desideratum in medicine, and we trust will continue to study still further the diseases of infancy and childhood, a pursuit for which he has shewn himself so well adapted. This work contains a vast quantity of accurate information, communicated in clear and unassuming language; and independent of its general value, it has a peculiar interest in presenting the most complete views of the German infantile therapeutics that has ever appeared in our language. The author, however, by no means neglects British authorities, but on the contrary, quotes repeatedly from Cheyne, Underwood, Clarke, Maunsell, and Evanson, &c. We recommend Dr. U.'s work in the strongest and most unqualified manner to every practical man in the profession.

A Philosophical and Statistical History of the Inventions and Customs of Ancient and Modern Nations in the Manufacture and Use of Inebriating Liquors; with the Present Practice of Distillation in all its Varieties: together with an Extensive Illustration of the Consumption and Effects of Opium, and other Stimulants used in the East, as Substitutes for Wine and Spirits. By SAMUEL MOREWOOD, Esq., Collector of Excise.

THIS volume forms an exception to the general character of the publications of the present day, being profound and erudite; no one who has not examined its contents, could believe it possible to collect such a body of interesting information on inebriating liquors; and none but a man devoted entirely to his subject, could have undertaken or supported the labour of comparing and collecting authorities from all sources, ancient and modern, calculated to illustrate the history of inventions and customs connected with the manufactures, the use, or the abuse of vinous and fermented drinks. We have not space to do more than make a few extracts, but we strongly recommend the work itself, which contains 750 closely printed octavo pages, to every one who wishes to get information on the innumerable expedients to which man, in every climate and in all ages, has had recourse for the purpose of procuring the means of refreshment, exhilaration, or inebriety.

We cannot resist the temptation of laying before our readers a few of the citations which our author has collected respecting opium eaters.

“ Dr. Poqueville, in his Travels through the Morea, gives a minute account of the opium eaters termed Theriakis, an appellation by which they are designated, in consequence of their being extravagant and irregular characters. ‘They begin,’ says he, ‘with only half a grain, and increase the dose, as they may find it to produce the desired effect. They take care not to drink water after it, as that would bring on violent colics, but the man who, at twenty, takes to opium, seldom lives beyond the age of thirty or thirty-six. In the course of a few years, the dose is increased to upwards of a drachm, or sixty grains. At this time, a pallid countenance and extreme leanness announce a state of cachexia, which is only a prelude to a general marasmus, or consumption of flesh. The infatuation is so great, that the certainty of death and all the infirmities that lead to it, is incapable of correcting a theriaki, or a person addicted to the use of opium; he coldly answers any one who apprizes him of his danger, that his happiness is incomparable, when he has absorbed his pill of opium. If he be asked to define this supernatural felicity,

he only says that it is impossible to describe it, as it is a pleasure not to be explained. These miserable beings, however, towards the close of their life, or rather of that state of stupefaction, into which they are plunged, experience the most severe pains, and a continued hunger; they are tormented by a desperate *satyriasis*, without the capability of satisfying their desires; in short, they experience pains which even the delicious paragoric cannot assuage; and having become hideous, deformed by numerous periostoses, deprived of their teeth, their eyes sunk into their head, and afflicted with an incessant trembling, they cease to exist a long time before their life is at an end. The Baron de Tott, writing on the same subject, gives a miserable picture of those who frequent the opium market, at Constantinople, describing them as having pale and melancholy countenances, with meagre necks, heads twisted to one side, backbones distorted, shoulders drawn up to the ears, and other extraordinary appearances. Seated in the twilight of the evening, or reclining on sofas in the little shops, ranged along the walls of the mosque of Solyman, may be seen the infatuated theriakis swallowing their opium pills, in proportion to the degree of want, which habit has rendered necessary. Each poor votary anxiously awaits the agreeable reverie that is to follow, as the effect of this indulgence. He soon retires to his home, full of an imaginary happiness which neither reason nor the realities of life can procure; and in this manner, each succeeding day witnessess a repetition of the same irregularity, till, worn out with debility and intemperance, he, at last, sinks like a shadow into the grave. In addition to these observations, the following anecdote will be read with interest:—An English ambassador, lately sent to a Mahometan prince, was conducted, upon his arrival at the palace, through several richly decorated and spacious apartments, crowded with officers arrayed in superb dresses, to a room, small in dimensions, but ornamented with the most splendid and costly furniture. The attendants withdrew. After a short interval, two persons, of superior mien, entered the saloon, followed by state-bearers, carrying under a lofty canopy a litter covered with delicate silks, and the richest Cashmere shawls, upon which lay a human form to all appearance dead, except that its head was dangling loosely from side to side, as the bearers moved into the room. Two officers, holding rich fillagree salvers, carried each a chalice, and a vial containing a black fluid. The ambassador, considering the spectacle to be connected with some court ceremony of mourning, endeavoured to retire; but he was soon undeceived by seeing the officers holding up the head of the apparent corpse, and after gently chafing the throat and returning the tongue, which hung from a mouth collapsed and gaping, pouring some of the black liquor into the throat, and closing the jaws until it sunk down the passage. After six or seven times repeating the ceremony, the figure opened its eyes, and shut its mouth voluntarily; it then swallowed a large portion of the black fluid, and, within the hour, an animated being sat on the couch, with blood returning into his lips, and a feeble power of articulation. In the Persian language he ad-

dressed his visiter, and inquired the particulars of his mission. Within two hours this extraordinary person became alert, and his mind capable of arduous business. The ambassador, after apologizing for the liberty, ventured to inquire into the cause of the scene which he had just witnessed.

“ Sir,” said he, “ I am an inveterate opium-taker; I have by slow degrees fallen into this melancholy excess. Out of the diurnal twenty-four periods of time, I continually pass eighteen in this reverie.—Unable to move, or to speak, I am yet conscious, and the time passes away amid pleasing phantasies; nor should I ever awake from the wanderings of this state, had I not the most faithful and attached servants, whose regard and religious duty impel them to watch my pulse. As soon as my heart begins to falter, and my breathing is imperceptible, except on a mirror, they immediately pour the solution of opium into my throat, and restore me as you have seen.—Within four hours I shall have swallowed many ounces, and much time will not pass away, ere I relapse into my ordinary torpor.”

We must close this brief notice with a diverting account of a peculiar fungus.

“ The effects of the *Amanita Muscaria*, a species of reddish fungus, or mushroom, plentiful in different parts of the Russian empire, and in Kamstchatka, where it is called *mouch-more*, are familiar. The account given of it by Dr. Langsdorff, a Russian physician, is worthy of recital. ‘The *Amanita Muscaria*, so called from its power of killing flies, when steeped in milk, though the most poisonous of our fungi, is used by the inhabitants of the north eastern parts of Asia, in the same manner as wine, brandy, arrack, opium, &c. are used by other nations. It is collected in the hottest months, and dried by being suspended in the open air; some found on the ground, naturally dry, is esteemed as the most powerful narcotic. The common mode of using it is, to roll it into the form of a bolus or pill, and swallow it without chewing. It is frequently eaten dry, but oftener taken when infused in a liquor made with *epilobium*. It is sometimes eaten fresh in soups and sauces, and in this state loses much of its intoxicating property. When steeped in the juice of the berries of the *vaccinium uliginosum*, its effects are similar to those of strong wine. One large fungus, or two small fungi, is a common dose, to produce a pleasant intoxication for a whole day, particularly if water be drank after it, which augments the narcotic principle. From one to two hours, after taking the dose, giddiness and drunkenness ensue, cheerful emotions of the mind are the first symptoms, the countenance becomes flushed, incoherent words and actions follow, and sometimes a total want of consciousness. It renders some very active, and proves highly stimulant to muscular exertion. Too large a dose brings on violent spasmodic affections, and such are its excitements on the nervous system, that it renders many very silly and

ludicrous. If a person, under its influence, wish to step over a straw, or a small stick, he takes a stride or a jump sufficient to clear the trunk of a tree; a talkative person can neither keep secrecy nor silence, and one fond of music is perpetually singing. The most extraordinary effect of the amanita, is the change it makes in the urine, by impregnating it with an intoxicating quality, which continues to operate for a considerable time. A man moderately intoxicated to-day, will, by the next morning, have slept himself sober; but, as is the custom, by drinking a cup of his own urine, he will become more powerfully intoxicated than he was the day preceding. It is therefore not uncommon for confirmed drunkards to preserve their urine as a precious liquor, lest a scarcity in the fungi should occur. This inebriating property of the urine is capable of being imparted to others, for every one, who partakes of it, has his urine similarly affected. Thus with a very few amanitæ, a party of drunkards may keep up their debauch for a week.' Dr. Langsdorff states, that by means of a second person taking the urine of the first, the third that of the second, the intoxication may be propagated through five individuals. The relation of Strahlenberg, that the rich lay up great stores of the amanitæ, and that the poor, who cannot buy it, watch their banquets with wooden bowls, in order to procure the liquor after a second process, is fully confirmed by the statement of Langsdorff, and gives a lamentable picture of the debasement of our species in that quarter of the world."

Fig. 1

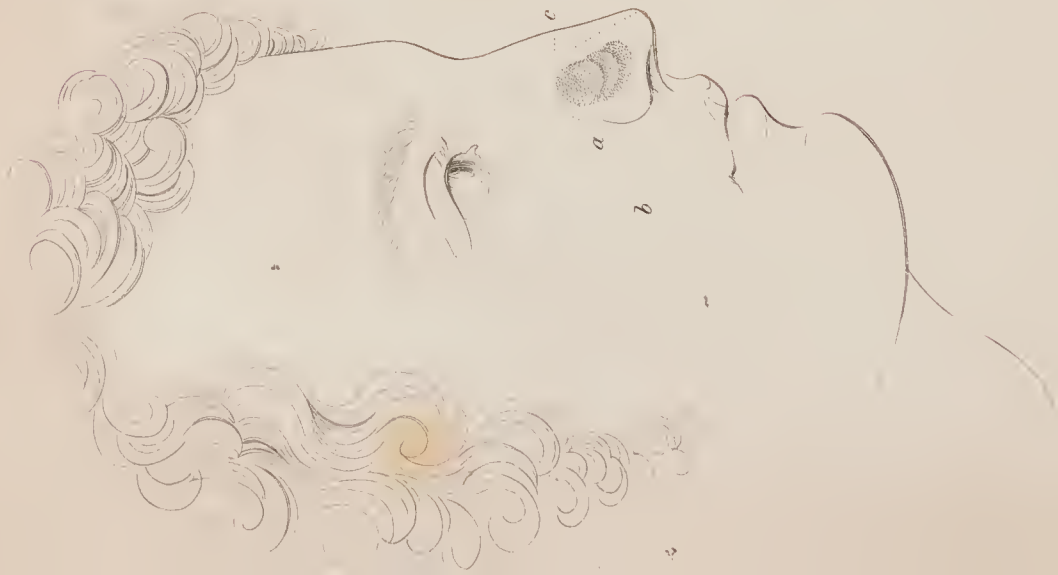


Fig. 2



Fig. 3



Fig. 4



SCIENTIFIC INTELLIGENCE.

Novel Rhinoplastic Operation, by Thomas D. Mütter, M.D. Lecturer on Surgery, &c.—About the 1st of October, 1837, I was requested by professor Jackson to visit, in consultation, a gentleman from the South, who had had the misfortune to lose a considerable portion of the right half of his nose.

Without entering into a history of the case, which would have but little bearing upon the operation to be described, I shall proceed at once to state the character of the deformity and the means employed to accomplish its cure.

Upon a reference to the plate, *Fig. 1*, it will be seen that the whole of the right ala, as well as the adjacent soft parts, as high up as the os nasum of the same side, are wanting. As a consequence of this loss, an opening half an inch in its *perpendicular* diameter, and about three-quarters of an inch in its *transverse*, at the widest part, and of the shape represented in the sketch, was established. The margins of this opening were thin and callous, while the neighbouring tissues, to the distance of two or three lines, were much paler and firmer than natural, owing to the deposit of lymph, during the period of inflammation to which they had recently been subjected.

The septum nasi, the ossa nasi, and the Schneiderian membrane were perfectly sound. The face was rather full, and its integuments healthy, with here and there a small cicatrix, the result of previous local inflammation.

As the deformity was striking, and as the deficiency of nostril on one side modified the voice, so as to render it rather disagreeable, the patient determined to submit to any operation that promised success. His general health, though delicate for some years past, is at the present moment excellent; while his age (28,) and temperament (sanguine,) rendered our prognosis, respecting the results of an operation, very favourable.

Not wishing, however, to take the whole responsibility of an operation, so serious in its bearings, upon myself, I requested the con-

sultation to be extended, and accordingly Drs. T. Harris, J. Randolph, and J. R. Barton were called in.

Upon an attentive examination of the deformity, it was determined to attempt its relief by an operation differing essentially from those proposed by the surgeons who have devoted their attention particularly to the autoplasic department of surgery. The details of this operation I shall now present. It was performed on the 6th of October, at 12 o'clock, in the presence of Drs. Jackson, T. Harris, J. Randolph, J. R. Barton, P. B. Goddard, and Langley:

The patient was seated with the organ to be operated upon exposed to a good light, while his head was slightly thrown back, and supported by Dr. Randolph. Seating myself in front, I commenced the operation by making, with a small sized convex edged bistoury, an incision extending from a few lines above the *superior* border of the orifice, to a short distance *below* its inferior, and directed *downwards* and *outwards*. (See Plate, *Fig. 1, a.*) It did not penetrate to the bone, but was sufficiently profound to allow a flap about *three* lines in thickness to be readily detached. Upon reference to the plate it will be found, that this incision was completely on the *outside* of the cicatrice, a portion of which was subsequently removed in order to prevent its hardened edges from irritating the raw surface of the flap, which was to be placed immediately upon it.

One or two small arteries were cut across, but the hemorrhage from them was arrested by pressure, until the *second* incision was made. This commenced at the *terminal* extremity of the first, and *extended horizontally outwards about an inch*. (See Plate, *Fig. 1, b.*) A *triangular* flap was thus marked out, and immediately detached, from the subjacent bone, by dissecting with the edge of the knife held nearly parallel to the surface of the cheek. In the execution of this part of the operation, two or three arteries of some size were necessarily cut across, and required the application of the ligature.

The *third* incision, which extended from the *initial* extremity of the *first* to the point of the nose, (see Plate, *Fig. 1, c.*) was made with a pair of strong straight scissors, these being preferred to the scalpel, in consequence of this margin of the orifice being, to a certain extent, loose and unsupported. The triangular piece of cicatrix included between the superior extremities of the first and third incisions, was then removed with the scalpel and forceps; and the sharp margin of the inferior portion of the opening also pared off, for reasons already stated.

The hemorrhage having been arrested, and the parts properly sponged, the next step of the operation was undertaken. This consisted in the approximation of the first and third incisions, and the application of such measures as were calculated to retain the flap in its proper position. From the free dissection, and the yielding character of the subcutaneous cellular tissue of the cheek, no difficulty was experienced in placing the edges in contact, and in order to insure their perfect and close approximation, *four* stitches of the *inter-*

rupted suture made with the sadler's silk, waxed and doubled, were passed. (See Plate, *Fig. 2.*) In addition, two or three small adhesive strips were applied to the spaces between the sutures.

Finally, in order to prevent adhesion between the *flap* and raw surface beneath it, and to give a better shape to the former, a small roll of soft lint, well oiled, was introduced into the *new nostril*. (See Plate, *Fig. 2, d.*)

The patient bore the operation, which was necessarily tedious and painful, with remarkable firmness. He was ordered *tinct. opii*, *gtt. xxx.*; to be kept perfectly quiet, and to lie with his *head elevated*. The temperature of the room to be 50° Fahrenheit.

[Reports are given for the next five days, which we shall not insert.]

In consequence of the *contraction* of the flap, the septum nasi was caused to incline to one side, which deformity was made very evident by the removal of the granulations.

In order to remedy this, the *line of union between the base of the flap and the cheek* was divided, cutting from within with a small scalpel held parallel to the surface of the cheek, to the extent of three or four lines.

The plug was then increased in size, and introduced into the nostril, while a wide adhesive strap was carried from the tip of the nose across the cheek on the sound side, and attached just in front of the ear, in order to incline the septum in this direction as much as possible.

No bad consequences resulted from this operation, and the antiphlogistic system, to a certain extent, was pursued for a couple of weeks longer. The patient's diet, however, had been somewhat improved, and he had also been allowed to move about the house. The use of the strap across the cheek was discontinued, in consequence of its invariably producing irritation of the skin. It moreover exerted but little influence in straightening the septum. The plug, during this period, had been gradually increased in size, with the view of *distending* the nostril, as well as to give it a proper "*set*," and the granulations within, which were very luxuriant, touched twice a day with a weak solution of *nit. argent.* or creosote, and occasionally with solid caustic; at the end of the sixth week from the day of the first operation, it was determined to execute the "*third step*" in the treatment. This consisted in the division of the skin and cellular tissue at the base of the flap in a *semi-circular* direction, the convexity of the curve looking outwards. The object of this incision was to give the peculiar rounded margin of an original ala; to diminish the fulness of the cheek where the natural depression should exist, which depression had of necessity been destroyed by the tension of the flap, and to permit a return to the perpendicular position of the deviated septum nasi. The incision was made with a small scalpel, and extended to the depth of three lines. In order to prevent union of its margins, a small roll of oiled lint was introduced into the cut, and a strip of adhesive plaster applied to the tip of the nose,

and fastened on the cheek of the sound side. (See Plate, *Fig. 3, a*, for shape of incision.) The patient was ordered to confine himself to his room, and to reduce his diet.

On the third day the dressings were removed, and it was found that the margins of the incision were nearly cicatrized and beautifully rounded off.

The same dressing was reapplied, and the plug intended for both incision and nostril increased in diameter. No change of importance was made in the dressing, or in the subsequent general treatment, except that the patient was allowed to return to a more generous diet, and to take exercise in the open air. At the expiration of the eighth week the nose presented the appearance exhibited in *Fig. 3*. The contraction of the granulations had caused the margin of the flap to be rounded off, and the cicatrix resulting from the union of the first and third incisions, which was originally located nearly upon the dorsum nasi, to descend nearly to the cheek. This latter change was a very favourable circumstance, as it produced a depression in the exact spot at which it was required, in order to give a proper expression to the face. Had it not taken place, there would have remained *a sort of inclined plane* from the bridge of the nose to the outer portion of the cheek. At the expiration of the ninth week, my patient returned home with scarcely a vestige of his deformity remaining. There existed a slight deviation of the septum, but this was perceptible only on close examination, and in all probability will gradually diminish, as the tissues of the cheek regain their original elasticity. There was also a slight discharge of mucus and pus from the nostril, owing to a few of the granulations being still uncicatrized.

The nostril itself is perfectly open, and its orifice nearly of the shape of its fellow.

The flap presents the usual colour of the skin of the face, and is so firm, that the patient unhesitatingly made use of it in the ordinary operation of cleansing the emunctory. In short, as was remarked by one of the attendants, "so perfect is the cure, that no one would ever imagine that an operation had been performed upon the organ." The voice was also rendered natural.

Remarks.—With those familiar with the divisions of modern "*Autoplastie*," the operation just detailed will readily be recognized as belonging to that, in which the loss of original tissue is supplied by *sliding a portion of neighbouring integument over the deformity*, (*operation par glissement du lambeau.*) For the *principle*, the profession is indebted to Celsus.

In cases similar to that of my patient, there cannot be a doubt of the vast superiority of this operation over all others hitherto performed, and the wonder is, that it has never, so far as I am able to learn, been before resorted to. Dieffenbach, Graafe, Labat, Dupuytren, Blandin, Liston, and every other authority, ancient as well as modern, that I have consulted, make no mention of such an operation for *such a case*. The *principle*, it is true, has been applied to other

cases of deformity ; for example, a fistula of the male urethra was cured by Alliot, by “ *sliding* a portion of sound skin over the opening, and then uniting it by sutures to the surrounding parts.” Chopart, Roux, de Saint Maximin, Lisfranc, Velpeau, Blandin, and others, have also made use of in a variety of cases, but more especially in ulcers of the *cheek* and *lips*.

The mode of relieving the deformity created by the loss of one ala nasi, has heretofore consisted in the section of a flap from the cheek, the *pedicle* of which rests on the *margin* of the wound. *Torsion* is resorted to, and the *flap* attached to the septum, &c. by suture or strap. Another plan, practised especially by *Liston*, and the English generally, has recently been published. This consists in the section of a flap of the form represented in Plate, *Fig. 4* ; and is altogether a better operation than the one usually performed. There is here no *twisting* of the pedicle ; “ the coaptation of the flap is consequently more exact, the supply of blood more free, and the vitality of the part less endangered.”

A, form of flap on cheek ; B, the slip of attachment. It is evident that by simply bringing A into a straight line with B, the flap may be placed in apposition, without any twisting of the attachment ; the acute angle between the two being entirely removed by the change.—*American Journal of the Medical Sciences*, May, 1838.

Use of Musk in some Diseases of Children.—It is extensively employed on the Continent in the following ailments incidental to infancy and childhood :

1. In the advanced stages of adynamic and ataxic fevers, and acute exanthemata, when, from exhaustion of the nervous system, the restorative process is suspended, and fatal sinking or dangerous metastases to important organs impend. Here, no time ought to be lost in resorting to its exhibition ; giving it in full doses, and enhancing its agency by other appropriate remedies, as ammonia, camphor, and warm baths.

2. In phlegmasiæ, when there exists a high degree of enervation, inducing convulsions, or complicated with them. Hence, in two of the most deadly scourges of infancy, acute hydrocephalus and croup, its use is indicated ; but under circumstances, whose discrimination requires the utmost tact on the part of the medical attendant : both of these diseases are in their nature and origin essentially inflammatory : both demand a strictly antiphlogistic treatment, by which alone can a cure be accomplished. Should they therefore be ushered in by convulsions, no unfrequent occurrence, such a phenomenon by no means calls for the administration of antispasmodics, but rather an opposite medication, one appropriate to arrest the phlegmasia, of which they are but the outward visible sign. If, however, in the hydrocephalic affection, after the leading indications have been fulfilled, stupor persists, sensibility being oppressed, and spasms betoken profound cerebral disturbance, (Wendt ;) or if in

croup, after inflammation has been subdued, and the exuded coagulable lymph evacuated, convulsions continue or become developed, causing dyspnœa and asphyxia; then is the cautious exhibition of antispasmodics, and above all musk, equally appropriate and efficacious. Experience uniformly proves that, in infancy, the transition from a state of active inflammation to one of vital exhaustion, is often most sudden; especially where energetic depletion has been pursued. Whence musk constitutes a remedy the more valuable, inasmuch as it tends to counteract the pernicious after-consequences of that loss of blood, which the urgency of symptoms at the beginning of the attack rendered indispensable. Under such circumstances it may be combined with calomel or antimonials; partly to check any latent inflammation, and partly, in the instance of croup, to promote expectoration. Some physicians, observes Fränkel, from whom we have borrowed the foregoing details, have ordered musk in the early stage of croup, and with the best results, even without any abstraction of blood. Thus Wigand gave, according to the age and constitution of the little patient, from two to five grains of calomel with half a grain or a grain of musk every hour, until distinct vomiting of mucus succeeded the rattling in the throat and trickling from the mouth. It appears however, that many of the cases adduced were not really croup; and other experienced practitioners, having tried it, deprecate this method as fallacious, recommending in every instance of well marked sthenic croup, occurring in a plethoric habit, copious withdrawal of blood. Yet, in cases where the child is of a weakly and relaxed constitution, says Sachse, where a simple mercurial medication will suffice; and where the prevailing spasmodic symptoms require to be allayed, then the above combination of musk and calomel may be profitably exhibited.

In a treatise, just published by Dr. Seifert of Greifswald, on the bronchiopneumonia of infants, the remedial efficacy of musk in that complaint is particularly insisted on. After the system has been reduced by abstraction of blood, should considerable general feebleness be present, as denoted by remarkable pallor and diminished temperature of the surface, progressive collapse and sunken features, and accompanied with more or less manifest decline of the fever, the exhibition of musk ought to be commenced and steadily persevered in until convalescence be established. He prescribes it in the dose of one or two grains to infants a few weeks old; and continues it every hour or two hours, sometimes for ten or fourteen days. He gives it by itself, in linctus, and promotes its operation by warm baths.

Horn says, "I have employed this substance (musk) particularly in the pneumonic affections of children very frequently, and almost uniformly with signal benefit." And elsewhere, "I believe that I do not speak extravagantly, when I attribute the cure of numerous cases of pneumonic disease in children to the early and liberal

employment of musk.”—(Die Erkenntniss und Heilung der Pneumonie, Frankfurt, 1802.)

Frank speaks of it in the following emphatic terms. “Præsertim in bronchitide typhode miracula vidi (sc. ex. moscho.”) Vid. Praxeos Medicæ universæ præcepta, partis ii. vol. ii. sect. i. pag. 400, nota 28.—*Ure’s Practical Compendium of the Materia Medica.*

Animal Magnetism.—[We feel greatly rejoiced, that the Editor of the Medico-Chirurgical Review has so ably exposed the disgusting quackery at present going on in London. We wish we could insert the whole of the article from the last number, of which the following extract is the conclusion.]

We have now displayed the principal phenomena resulting from the employment of animal magnetism. A slight analytical examination of them may not be uninteresting.

a. In the first place, we may remark that nothing is effected by the agency of mesmerism, which the magnetisers themselves do not proclaim to have happened again and again independently of its assistance. The sceptic suspects, perhaps unjustly, the occasion of this strange anxiety to underrate their art. He sees, or thinks he sees, that, ashamed of its monstrosities, and alarmed at its absurdities, they are willing to blind the public eye with the dust of like absurdities, and like monstrosities. Be that as it may, we observe in this respect a singular analogy between mesmerism and astrology.

b. Although the phenomena of mesmerism are all wonderful, they admit of being resolved into two distinct sets. The first are simply wonderful; the second simply impossible.

The extraordinary phenomena, however extraordinary, are still not contrary to reason. If the magnetisee falls into a species of trance, raves in delirium, or is agitated by convulsions—nay, if, like the priestess of Delphi in former times, and the Irvingite fanatics in our own, she affects to foresee and to foretel the future,—there is nothing which might not readily be generated from a heated imagination, acted on by *any* powerful impression. The very pains which are taken by the magnetisers to insist on the frequency of such phenomena, independently of magnetism, prove, if they prove any thing, that it is the nervous system of the individual acted on, and not any special power in the agent, which gives those phenomena birth. It is true that they now appear in a monstrous and exaggerated form. But the collateral circumstances are monstrous and exaggerated too. If our ordinary hysterical patients do not “go the whole hog,” like Okey, their antics, it must be remembered, are *checked*, not *encouraged*. Give hysteria license—pat it on the back—exhibit it—and offer the premium that the magnetisers do offer, on its fancies, follies, and cheats, and a thousand Okeys would soon be in the field. But if Okey received her deserts, and had a pailful of cold water emptied on her once or twice, as many an honest woman has had, we would soon find that brooches would not be taken for oysters, and

her vulgar slang would be kept in the pure recesses of her own bosom, not poured into the ears of credulous marquises and gaping dukes.

c. The clair-voyance, the transposition of the senses, the prevision, are—"Fudge." Those who can believe any or every thing, will probably believe them. The patrons of quackery, and they are many—the proselytes of homœopathy, and they *were* a few—the lovers of the marvellous in all its forms, from the man in the bottle to the magnetic somnambule—the thirsty hunters for novelty—all these will credit the possibility and the fact, of God permitting his demonstrable and eternal laws to be reversed, on the most ridiculous occasions, for the most ridiculous of purposes, by the most ridiculous of means. They will believe that any impudent strumpet may get a permit from heaven to exhibit at half-a-crown a head, vision with her fingers or her toes, and may be allowed to read the mystic scroll of the future, which God has wisely hid from the penetrating gaze of science, and the humble wish of virtue. It would indeed appear that—

The pleasure is as great
Of being cheated as to cheat.

They who can credit such gross delusions are beyond the reach of argument. Ridicule is the only method of correction. If they feel it, there are hopes. If callous to it, they would do well to "purge and let blood" when the moon's at full.

It would be hard to predict how long this epidemical folly will last. As the dog days are approaching, it will rage, of course, for a while longer. It will count its votaries till some new absurdity, fresher at all events, if not more monstrous still, appears on the horizon. The stock of credulity in this world is inexhaustible. Reason, experience, are opposed to it in vain. The wonder-monger, like the gambler, plays deeper the more he has lost. The next throw, he may win.

Dr. Clutterbuck versus the Stethoscope. Dr. Hope on Auscultation in Valvular Disease.—In the heading of this notice, we have connected the names of the above gentlemen, because, although they are in no way related as authors or colleagues, they have (no doubt unwittingly) joined to injure the cause of auscultation and of science, in their late publications: the one, by attempting to detract from its value; the other, by ascribing to it powers which it does not possess.

In the London Medical Gazette for July 28th, 1838, we have a lecture of Dr. Clutterbuck's on the treatment of periodical asthma, and on blood-letting in the specific inflammations of the chest. In this lecture, the following irritable effusion appears.

"I may take this opportunity of adverting to the method of investigating diseases of the thorax by auscultation; that is, by listening attentively to the sounds emitted during respiration; and also by sounding the cavity, by tapping with the ends of the fingers on different parts of the chest. This mode of examination has always

been resorted to more or less by physicians ; though, from the employment of a load of new terms, invented chiefly by our ingenious neighbours the French, and introduced by some of our own practitioners who have enjoyed the advantages of the Parisian schools, one would be led to suppose that a new region of science had been discovered, not inferior to mesmerism or homœopathy. As a specimen of the new language introduced on the occasion, I may enumerate the following, indicating, it is supposed, as many various conditions of the organs in question. Thus, in the compass of a few pages you will meet with the following :—‘ Pectoriloquy, perfect and imperfect’ — ‘ bronchophony’ — ‘ pneumo-thorax’ — ‘ rhonchus’ — ‘ cre-pitation, fine and coarse’ — ‘ vocal resonance’ — ‘ tinkling echo’ — ‘ metallic tinkling’ — ‘ amphoric, or bottle like sound’ — ‘ clicking’ — ‘ bubbling’ — ‘ gurgling’ — ‘ snuffling’ — ‘ whiffs of a cavernous respiration’ — ‘ fistular resonance, like that of a pan-pipe or key’ — ‘ pectoriloquy, forming a little island of voice’ — *cum multis aliis.*”

Dr. Clutterbuck seeks to destroy the fame of Lænnec by the worn out system of denying his originality. Can he point out a single author, who used auscultation as Lænnec did, from the time of Hippocrates to the discovery of the stethoscope? He cannot. He is strangely ignorant, when, combining the modes of auscultation and percussion, he states that “ *this mode has always been resorted to by physicians,*” and his joke about auscultation as equal to mesmerism and homœopathy, comes with a bad grace from one, himself the author of an unphilosophical and exploded theory of fever.

But Dr. Clutterbuck is an auscultator. He can tell by “ *the tone of the cough* whether there is not a great cavity in the lungs, the result of suppuration or ulceration.” He can tell with “ tolerable precision, whether a quantity of mucus lies loose and floating as it were in the air tubes !” He can judge of the state of the larynx by the sound of the voice ; and ascertain whether the lungs are pervious to air. His powers of diagnosis are certainly great ; his opinion in chest disease, must be equally valuable.

We suspect Dr. Clutterbuck’s sense of hearing must be injured ; for to him the “ *ear trumpet*” magnifies but distorts the sound, rendering it less distinct than before. He holds that it may be classed with the telescope and the microscope, and includes all three in his anathema ! And he adds, that “ the information thus acquired, supposing it to be correct, comes too late in general to be of any practical use. It serves to indicate the consequences of disease, rather than disease itself, and that at a period when they are far beyond the power of art to remedy.”

It is not true that auscultation only detects fully formed diseases. Its chief value is the facility with which it enables us to recognize the true nature of pleurisy and pneumonia, often a few hours after they have commenced, and consequently at a time when the knowledge thus obtained leads to the almost instant arrest and cure of the disease.

We would ask Dr. Clutterbuck, whether it is of no practical use

to discover an apyrexial hepatization, to distinguish between this and a circumscribed pleuritic effusion? to discover whether, in a case of laryngeal disease, the lungs are healthy or diseased? to distinguish between an empyema with or without a pulmonary fistula? to detect a foreign body fixed in the bronchus? to distinguish, in a case of stridulous breathing, where tracheotomy is apparently called for, between tracheal disease and the pressure of an intra-thoracic tumour? to detect the existence of effusion into the pericardium? or to discover latent disease of the mucous membrane, parenchyma, or serous structures in a case of typhous fever? We might add an hundred more of such instances.

Let us be clearly understood. We write these remarks for the junior student, who might be deterred from studying an important and now indispensable part of his profession, by the statements above quoted. We seek not controversy with Dr. Clutterbuck, his opinions can only affect the uninformed.

In the next number, Dr. Hope, of whom we wish to speak with the respect which his labours have earned for him, has authorized the publication of a series of diagnoses, made by his pupils after a ten minutes' lecture on the most difficult part of medicine, namely, the valvular diseases of the heart. The pupils were inexperienced, and as far as we can learn, availed themselves solely of physical diagnosis. Their conclusions, in thirteen cases out of fifteen, were "*correct*;" although they had, amongst others, to deal with the rare diseases of the pulmonic orifice.

That the pupils, after having been instructed in Dr. Hope's views of the causes and situations of valvular murmurs, should have come to conclusions such as he would have done, is not wonderful; but that these conclusions were correct, we have only Dr. Hope's word for. We shall not examine into the evidence of the conclusions, for we know it to be insufficient; but we object to the whole proceeding, as calculated to revive the often repeated and refuted objection to the advocates of auscultation, that they neglect the history of the case and vital phenomena.

The following considerations we wish to impress on the pupils of the Meath Hospital.

First. That the physical signs of valvular disease are not yet fully established.

Second. That taken alone, they are in no case sufficient for diagnosis.

Third. That even in organic diseases *the nature and situation* of murmurs may vary in the course of a few days.

Fourth. That all varieties of valvular murmurs may occur *without* organic disease.

Fifthly and lastly. That organic disease of the valves may exist to a very great degree without any murmur whatsoever.

Of this assertion we shall hereafter bring abundant proofs.

R. J. GRAVES.

W. STOKES.

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1 NOVEMBER, 1838.

PART I.
ORIGINAL COMMUNICATIONS.

ART. VIII.—*A Reply to Dr. COLLINS'S last Communication in the Dublin Journal of Medical Science.* By JAMES HAMILTON, M. D., Professor of Midwifery in the University of Edinburgh.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

23, St. Andrew's-square, August 4th, 1838.

SIR—I hope that you will permit me to make a few observations on the article by Dr. Collins, in your last number, for that article contains certain statements calculated to misrepresent my opinions on some most important practical subjects.

The doctrines at issue between Dr. Collins and myself relate to the management of the first stage of all labours, and to the appropriate treatment of cases of laborious labours. I shall, therefore, notice Dr. Collins's animadversions on those subjects under two separate heads.

MANAGEMENT OF THE FIRST STAGE OF LABOUR.

It must be well known to the profession at large, that in a great proportion of cases of human parturition, (perhaps in eighteen or nineteen out of every twenty,) the contractions of the womb complete its dilatation, or what is called the first stage of labour within ten or twelve hours.

A question therefore naturally arises, whether in the cases of exception, the sufferings of the woman ought to be allowed to proceed, or whether the assistance of art to alleviate or shorten those sufferings, can be safely and successfully interposed. On the decision of this question, the controversy on which Dr. Collins has volunteered must depend.

The result of my experience has convinced me, in the first place, that the protraction of the dilatation of the mouth of the womb beyond the ordinary period, is not unfrequently productive of much injury both to the mother and to the infant; and secondly, that there are certain safe means by which such protraction can be prevented.

Every candid reader who will take the trouble to consider attentively what I have published on this subject, in Part first of my "*Practical Observations*," from page 211 to page 236, must admit, that such are the propositions on which I have founded my directions for the management of the first stage of labour.

But Dr. Collins has, in the *Dublin Journal of Medical Science*, No. XXXI., page 39, given a very different view of my doctrine. He says, "Dr. Hamilton declares that the first stage of labour, viz. the full dilatation of the os uteri should be completed within twelve or fourteen hours from the actual commencement of labour, as the natural efforts can no longer be trusted to; that sundry measures are to be resorted to by the medical attendant for this purpose, and that the patient should, almost never, be allowed to continue longer than twenty-four

hours without being delivered. The following are his own words: 'When the pains take place, if the dilatation prove tedious, that is, if the continuation of strong pains for *six* or *eight* hours do not advance the dilatation to such a degree as to give reason to expect its completion within a few pains, it becomes necessary to interfere, lest the patient's health should suffer.' "

In this quotation, Dr. Collins has left out certain words, and transposed others, which completely misrepresent what I have published; for firstly, I do not state "that the full dilatation of the os uteri should be completed within twelve or fourteen hours from the actual commencement of labour, as the natural efforts can no longer be trusted to;" my statement being in the following words: "If uterine contractions continue regular, the full dilatation of the os uteri should be completed within twelve or fourteen hours, &c."

This condition, which Dr. Collins has carefully suppressed, changes altogether the proposition which he has thus alleged to be mine.

For this misrepresentation he can have no excuse, because in the first part of my "Observations," page 195, I have thus expressed myself: "Young practitioners, the author is aware, may be deceived in their estimate of the duration of the first stage, especially in cases where the woman has had a family, for spurious pains are apt to precede the true ones, not only for hours but for days. Unless there be a decided tightening of the edges of the os uteri during the pain, the labour has not commenced."

Again, that there might be no mistake in this important subject, I have, in page 222, used the following words: "The author is most anxious to explain to the junior part of the profession especially, what is meant by the protraction of the first stage, for he is every year called in to cases where great mistakes upon this point are committed, chiefly in consequence of supposing spurious pains to be the true pains of labour."

And further, in page 223, I have added, “ there is another source of error, for it is certainly possible that after the first stage is fairly begun it may be suspended for some hours, the uterine contractions no longer occurring. If, during the interval, there be no injurious pressure upon any part of the mother, the previous pains are not to be reckoned, but the duration of the first stage is to be taken from the recurrence of pains.”

The second misrepresentation of Dr. Collins, in the words quoted, seems to me still more inexcusable. I allude particularly to the following : “ He (meaning Dr. Hamilton) says, that the patient should almost never be allowed to continue longer (*viz.* than twenty-four hours) without being delivered. The following are his own words : when the pains take place, if the dilatation prove tedious, that is, if the continuation of strong pains for *six* or *eight* hours do not advance the dilatation to such a degree as to give reason to expect its completion within a few pains, it becomes necessary to interfere, lest the patient’s health should suffer.”

These words have been detached from the sentences which explain them, and afford one of the most perfect specimens on record of a deliberate intention to pervert and misrepresent the doctrines which Dr. Collins has undertaken to controvert, as the reader will at once perceive by the following extracts from my *Practical Observations*, Part first, page 223.

“ Premature rupture of the membranes, is an accident which in many cases can be neither foreseen nor prevented, as it may take place spontaneously before there be any contractions of the uterus. Although always an untoward occurrence, (for the reasons already specified,) especially in a first labour, it does not invariably protract the first stage, but if it be allowed to do so, the patient’s strength is sooner exhausted than in some of the other cases of protraction, because, after the discharge of the liquor amnii, the uterus acts with great force, which is apt to wear out the woman’s strength.

“ A young practitioner must therefore naturally wish to be

informed what is to be done in a case where the liquor amnii is suddenly discharged without previous pain. It is absolutely necessary to institute an examination, in order to ascertain first, if there be any progress in the dilatation, and secondly, if the position of the infant be natural. This is a duty which is always disagreeable to the patient, and is therefore often resisted, for it is not easy to make her understand the utility, or even the necessity of such an examination, but in general it is unsafe to dispense with the investigation.

“ When the pains take place, if the dilatation prove tedious, that is, if the continuance of strong pains for six or eight hours do not advance the dilatation to such a degree as to give reason to expect its completion within a few pains, it becomes necessary to interfere, lest the patient's health should suffer.”

These directions, so unequivocally stated to be applicable to a certain deviation from the ordinary process of labour, have been thus held out by Dr. Collins to be my practice in all cases of the first stage of labour. This mistatement, unintentional as I then supposed it, has been very pointedly brought under the notice of Dr. Collins, in my first Letter to the Editor of the London Medical Gazette, and yet he has neither corrected the misrepresentation nor has he attempted to explain it. Instead of this, he has in the article of the Dublin Journal of Medical Science, No. 39, still further misrepresented my doctrines, a circumstance I could never have anticipated, and which I shall now notice as briefly as possible.

He says, page 407 of your Journal, No. 39, “ *two* untoward circumstances are expressly set forth by Dr. Hamilton, as the necessary effects of the protraction of labour beyond the time specified by him, upon which entirely rests the validity of his reasoning. These constitute the second and third heads of his doctrines. We shall use his own words.”

These *five* lines contain *two* mistatements; for firstly, I enumerate not two but *four* untoward circumstances, as the effects of the protraction of labour beyond a certain period; and

secondly, I do not allege them to be *necessary* effects. The reader will at once judge, for the following are my words:— (See page 404 of your Journal, quoted by Dr. Collins himself:)

“ Firstly, the powers of the uterus *may*, in the second stage, be inadequate to the expulsion of the infant, with safety to its life, or to the future health of the mother.

“ Secondly, after the birth of the infant, the uterus *may* contract irregularly, so as to occasion the retention of the placenta.

“ Thirdly, after the expulsion of the placenta, the contractions of the uterus *may* be too feeble to prevent alarming hæmorrhage.

“ Lastly, supposing the patient to escape all these untoward circumstances, febrile or inflammatory affections of a most dangerous nature *may* ensue, from the previous protraction of pain, and irregular distribution of the blood.”

In all these four sentences describing the several consequences of the protraction of labour, I have used the word *may*, which I need scarcely observe Dr. Johnson defines “ *to be possible.*” If the word *must* instead of *may* had been employed, Dr. Collins’s inference would have been correct; but considering my expressions, he is not warranted to assume that I allege the untoward circumstances enumerated to be the *necessary* effects, for I have most particularly declared my opinion to be, that they are the *possible* effects.

In the sentence preceding the above quotations, there is in my article in your Journal, No. XXXVIII., page 202, the following observation: “ I have stated in my Practical Observations, that the following are the *necessary* effects of the protraction of that process beyond the time specified;” but on turning to the passage of the Practical Observations alluded to, it will be found that my words are, p. 191, “ the following consequences may be dreaded, &c.”

Again, in page 387 of my first letter to the Editor of the London Medical Gazette, I have said:

“For nearly fifteen years I ascertained, or supposed that I had ascertained, that in all cases of *tedious* labour, where there was no actual disproportion on the part of the mother (with the exception of monstrosity or hydrocephalus, or wrong position of the infant), the most pregnant cause of the increased sufferings of the patient was the undue protraction of the first stage; and I became quite convinced that the effects of that protraction were the following.”—Here I added the four sentences already quoted, page 186.

Thus the word *necessary* is not to be found in the original work referred to, nor in my explanation of my doctrines in the London Medical Gazette. It had been inserted *by mistake* by the person who copied the manuscript for the article in your Journal, as must be quite evident to any attentive reader, for it is expressly at variance with the subsequent sentences, and with the whole tenor of my opinions; I protest, therefore, most solemnly against the advantage which Dr. Collins has taken of this error of a transcriber.

This explanation renders it unnecessary for me to follow Dr. Collins's reasoning, p. 412, *et seq.*, Dublin Journal, No. XXXIX. But it may be useful to the profession to point out the means by which he has contrived to render plausible his alleged refutation of my doctrines.

Not contented with asserting that I hold all the *possible* effects of the protraction of labour to be the *necessary* and *inevitable* ones; he has selected for illustration only *two* of those effects, viz. retention of the placenta, and uterine hæmorrhage after delivery; and because those occurrences were rare (not that they did not happen) in the Dublin Lying-in Hospital, he has triumphantly appealed to this fortunate coincidence as completely proving the error of my opinions.

Of the other injurious effects of protracted labour which I have stated, viz. the death of the infant, and future organic diseases, and febrile and inflammatory affections of the parent, he has taken no notice, conscious as he must be, that such were

really, in many cases, the consequence of protracted labour even in the patients of the Dublin Lying-in Hospital.

A third misrepresentation must strike the attentive reader : Dr. Collins says, p. 405 of your Journal, No. XXXIX., "I shall now as briefly as possible prove to the satisfaction of every thinking individual, and that from the actual results of sixteen thousand four hundred and fourteen deliveries, that where the patient is properly treated during the progress of labour, the mortality from the effects of protracted labour is strikingly small."

These observations are calculated to hold out that I had alleged that the protraction of labour beyond a certain time is productive of much mortality. And yet I have never used such a word. Instead of that, I have stated my belief (London Medical Gazette, for June, 1837, p. 300,) "that generally speaking, the pregnant women who resort to the Dublin Lying-in Hospital, and to the Lying-in Hospitals of Paris, are much more capable of enduring with impunity a protraction of labour, than women in the grades above them, reckoning from the wives of respectable tradesmen, up to ladies of the highest rank. But I have stated strongly my conviction that the protraction of labour beyond the more ordinary period, may lay the foundation of organic diseases which may injure the future health, or may shorten life;" an observation of which Dr. Collins has taken no notice.

No more than *two* untoward circumstances resulting from the protraction of labour have been commented on by Dr. Collins, and yet I have enumerated *four* which he himself has quoted, pages 404-5.

Dr. Collins, the intelligent reader will see, had good reason for having passed over in silence the first of the untoward circumstances which I have specified, viz. "that the powers of the uterus *may*, in the second stage, be inadequate to the expulsion of the infant, with safety to its life or to the future health of the mother."

The fact is, that I have quoted from his own records, four cases, where inattention to promoting the dilatation of the os uteri evidently proved fatal to two of the women, and to all the four infants, viz. page 465, No. 210 ; page 471, No. 608 ; page 475, No. 725 ; and page 481, No. 1038 ; and it could be neither agreeable nor convenient for Dr. Collins to advert to those cases. I will venture to say, that excepting the case detailed by Professor Davis, p. 49 of his *Elements of Operative Midwifery*, as having occurred in an English workhouse, the medical annals of this empire do not record two more shocking instances of mismanagement than the cases narrated in his *Practical Treatise*, by Dr. Collins, page 465, No. 210 ; and page 481, No. 1038.

In the same way, Dr. Collins has taken no notice of the last of the untoward circumstances, which I have stated *may* be the consequence of the protraction of labour beyond the more ordinary period, viz. febrile or inflammatory affections of a most dangerous nature. That these effects of long continued labour may not occur so frequently in patients resorting to lying-in hospitals as in women in private life, may be readily conceded, and may be easily explained, but that they had occasionally appeared in the Dublin Lying-in Hospital cannot be doubted.

One of the most frequent remarks which I have heard young practitioners make, after reading Dr. Collins's *Practical Treatise*, is the total disregard to the *sufferings* of the poor patient, which is so little concealed, that provided the poor woman struggled through and left the hospital alive, it would seem that the intensity or duration of the pains she sustained were held of no account.

This disregard of the sufferings of the poor woman must not only have been most injurious to them, but also highly prejudicial to the public by the example held out to young men learning the profession. Every practitioner of midwifery should sympathize with the feelings of the patient, and should employ every safe means to alleviate and to shorten them.

It was long ago well observed by Dr. Osborne, that the calamitous condition of the female sex during the progress of parturition, is such that the humblest of them have the strongest and the most complicated claim upon the benevolence and skill of the practitioner. What would have been his feelings if he had lived to read the detail of some of the protracted cases recorded by Dr. Collins, such as that described page 465, No. 210.

In support of his objections to my doctrines, Dr. Collins has brought forward a witness on whose testimony he has placed great reliance, the anonymous author of an article in the *British and Foreign Medical Review*. But to that authority I most positively object. Would any jury listen to the testimony of a person speaking from behind a curtain?

There are more individuals than Dr. Collins who feel sore at the detection of their fallacies and sophistries, which my public duty as a Professor in this University sometimes compels me to lay before my pupils.

One of the most unpleasant tasks of a public teacher of any department of medical practice, is that of pointing out the errors of authors, and yet it is of the greatest importance, not only to medical students, but to the world at large. And although I have always endeavoured, in the exercise of this part of my duty, to comment on the opinions of others in the true spirit of giving instruction, and have anxiously avoided indulging in censure, I am well aware that offence has often been taken at my hesitating to adopt innovations which appeared to the authors to be most important suggestions. Your readers will at once understand that individuals thus criticised may gladly avail themselves of the shelter of a review to vent their spleen.

Dr. Collins has referred as a second objection to my doctrine, page 403, to the information communicated by myself in a private letter to him, on which he has most unceremoniously commented. Since that letter was written I have learned that I was much mistaken in supposing that some respectable students had taken amiss my observations on Dr. Collins's remarks. I had formed that inference from an anonymous letter, which I have

now reason to believe expresses the sentiments of a single individual.

Since Dr. Collins's article in your Journal, No. 39, appeared, several intelligent pupils, who have been from ten to twenty years in practice, have expressed their readiness to bear testimony to the utility and importance of the directions they had received (when attending my class) for the management of the first stage of labour.

I have, however, respectfully declined availing myself of such testimony, having no *personal* interest in the question. I have published the result of my experience for the benefit of the public, and feeling conscious that the principles and practice which I have recommended are calculated to alleviate the sufferings and to lessen the dangers of child-bearing, I leave those practitioners, who choose, to imitate the worthy bishop who continued to eat asparagus by the wrong end, to the indulgence of their own prejudices.

I have only one more remark to make on this subject. I have publicly and strongly stated, that no patient under my care, since the year 1800, in whom there was not a disproportion, has been above twenty-four hours in labour, and very few so long. Dr. Collins has not ventured in direct terms to controvert this assertion, but he has affirmed, page 40, Dublin Medical Journal, No. 31, that he has found *no fact* in my Practical Observations, establishing the validity of my doctrines; and he has in your Journal for July last, No. XXXIX. endeavoured by the *interpolation of words which I never used*, and by the most *palpable garbling of extracts*, to fasten upon me an accusation of want of veracity.

Having thus shewn by incontrovertible evidence, that Dr. Collins has totally misrepresented my doctrine, I beg leave to recal the attention of the reader to the real question at issue between us.

The propositions on which I have based my directions for the management of the first stage of labour are :

That in the cases which occasionally occur, where the first

stage is not completed within ten or twelve hours, notwithstanding regular uterine contractions, certain untoward consequences *may* be the effects of the protraction; and secondly, that there are certain safe means of facilitating the dilatation of the mouth of the womb, and of thereby lessening and shortening the sufferings of the patient.

Nobody could have anticipated, that in the present state of medical knowledge, a practitioner, who for seven years had held the responsible and highly honourable office of the Physician to the Dublin Lying-in Hospital, could have published two articles in a periodical Journal, designed to reprobate the practice founded upon those propositions, and who in the progress of his discussions could have thus expressed himself,—Dublin Journal, page 58.

“ In some instances, especially with first children, the mouth of the womb continues rigid and hot, with little tendency to yield under uterine action, accompanied not unfrequently by considerable irritation. In such, bleeding to the extent of ten or twelve ounces, and keeping the patient under the influence of slightly nauseating doses of tartar emetic (to which a small quantity of opium should be added) will be found to *promote relaxation*, and thus be *productive of the best effects*. In others, where a fold of the os uteri continues to be forced down before the head anteriorly, between it and the pubes, although elsewhere obliterated, the descent of the head will be much facilitated by applying two fingers to keep it stationary during the pain, and thus permitting the head to clear this obstruction; neither of these cases are often met with, nor have they any tendency to illustrate the opinions noticed. I make the observation here, having had *practical experience* of the *advantage* of the *treatment* ! ! ”

These words imply, firstly, an assent to those propositions, for he says that, “ in some instances the mouth of the womb continues rigid and hot,” and secondly, the admission that certain means (the very means I have recommended) “ will be

found to *promote relaxation*, and thus be *productive of the best effects*."

On this extraordinary inconsistency I avoid making those comments which I might fairly do from the Doctor's own declaration, "that in no single instance in all those cases, (viz. 16,414,) were any means whatever used to effect the dilatation of the mouth of the womb within any given period."

TREATMENT OF LABORIOUS LABOURS.

In my letters to the Editor of the London Medical Gazette, I shewed by incontrovertible proofs, that Doctor Collins had misrepresented my opinions on the following important points.

Firstly. That by quoting part of a paragraph, and by suppressing altogether my very minute directions for distinguishing cases belonging to the different orders of laborious labours, he has totally perverted my meaning. Journal, No. XXXI., page 40.

Secondly. That upon this manufactured misstatement he has founded the very serious charge, "that I advise the junior practitioner to be guided as to the safety or otherwise of his patient when in labour, by the *number of hours*," which is at direct variance with what I have published, as your readers will find by looking into my Practical Observations, Part II. pages 45, 46.

Thirdly. That he had, in page 41 of your Journal, No. XXXI. misrepresented my description of the phenomena of laborious labour, by interpolating the words *tedious*, and *an ordinary tedious labour*, words which in relation to laborious labours are not to be found in my Practical Observations.

Fourthly. That Doctor Collins has accused me in page 53, Dublin Medical Journal, No. XXXI., of having suppressed a most important part of one of his sentences, and asks "is it possible a more distorted view of our practice could be given than this quotation represents." The attentive reader will find the very words of which I am accused of having suppressed, printed accurately, pages 159 and 160 of my second part.

Fifthly. That in a note at the bottom of page 55, No. XXXI.

Doctor Collins has alleged that I had only used the forceps thirty-three times in forty-eight years, having suppressed the following important words, "*where I had had the charge of the patient from the beginning.*"

Sixthly. That in pages 51 and 52, Dublin Medical Journal, No. XXXI., by suppressing an essential part of a sentence, Dr. Collins has completely perverted my meaning, and has founded upon that perversion a very serious accusation. His words are these

"Doctor H., in noticing such cases, where the symptoms are urgent, states, that he should consider it his duty to relieve the poor woman, without paying the least regard to the condition of the infant;" again, "he cannot imagine a case of laborious labour, which had been much protracted, where the knowledge of the state of the infant can be necessary to regulate the practice." Dr. Collins adds, "these are startling observations to guide the junior practitioner;" and he refers to pages 104 and 107 of the second part of my Practical Observations.

Let me entreat the reader to turn up page 104 of Part II. of my Practical Observations, and let me direct his attention to the following words:

His practice in such cases, however, would be entirely directed by the state of the woman, and not by that of the infant. "If its head had been for twelve hours or more firmly compressed in the pelvis, not leaving space for the passage of a catheter; if the urine be retained from severe pressure on the urethra, the patient complaining of acute pain on pressure of any part of the abdomen, the pulse being at the same time hurried, and the strength failing;" he should consider it his duty instantly to relieve the poor woman without paying the least regard to the condition of the infant. Delay under such*

* The words thus marked by inverted commas are those of Dr. Collins, pages 16 and 17, of his Practical Treatise, and these words contain his description of cases, where he recommends the use of the perforator "sooner or later."

circumstances, according to Doctor Collins's own shewing, would be productive of sloughing of the contents of the pelvis, with all its fatal consequences, as he has so well described, page 13.

Again, on turning to page 107, the reader will find the following sentence: "He cannot imagine a case of laborious labour, which had been much protracted, where the knowledge of the state of the infant can be necessary to regulate the practice. *If the circumstances permit the safe use of the forceps, that instrument should be employed, admitting the necessity of interference, whether the infant be dead or alive. And on the other hand, if, from the previous mismanagement, or other circumstances, it would be unsafe to use that instrument, it ought not to be ventured upon, even though the infant be alive.*"

I now distinctly accuse Doctor Collins of having suppressed all the words in the preceding sentences which are printed in *italics*, and I confidently appeal to your readers, whether he has not by this suppression totally changed my meaning.

In page 422 of your last Journal, Doctor Collins accuses me of having selected *nine* cases, in which it was deemed advisable to effect delivery by the crotchet, the child's death having been ascertained by the stethoscope some hours previous; and he adds, page 423, the following words: "What, I ask Doctor Hamilton, was the result even of the nine cases chosen? I will answer; all but one perfectly recovered."

Dr. Collins has truly stated, that I have pointed out specially nine cases, where the poor women were allowed to suffer unavailing pain for hours after it had been ascertained by the stethoscope that the infant had been some hours dead. But he has suppressed the important fact, that I have *copied from his own records nineteen cases of that description*, and that *eight* of the women died. That there may be no misunderstanding, I now give a reference to the nineteen cases where the infants had been supposed dead, by the test of the stethoscope, many hours before the poor women were relieved.

In my extracts in the Dublin Medical Journal, I quote from Dr. Collins's Practical Treatise the following cases :

Page 158, No. 126, Died.	Page 169, No. 130, Recovered.
„ 300, „ 32, „	„ 471, „ 555, „
„ 464, „ 173, „	„ 471, „ 584, „
„ 473, „ 605, „	„ 472, „ 626, „
„ 473, „ 665, „	„ 474, „ 667, „
„ 477, „ 817, „	„ 475, „ 725, „
„ 481, „ 1038, „	„ 476, „ 740, „
„ 483, „ 1091, „	„ 478, „ 810, „
	„ 480, „ 976, „
	„ 480, „ 1032, „
	„ 482, „ 1041, „

Dr. Collins has ventured to accuse me, page 408, (Dublin Medical Journal, No. XXXIX.) of having “given a most distorted view of the facts and practices recorded in his Practical Treatise.” On this charge I join issue, and challenge Dr. Collins to show one misrepresentation of his facts and practice.

In the second part of my Practical Observations, I had made an erroneous reference to certain cases recorded by Dr. Collins, and whenever he pointed it out to me I explained, apologized for it, and corrected it, as acknowledged by Dr. Collins himself in your last Journal. But Dr. Collins has neither explained nor corrected the mistatements of my doctrines, which I have thus pointed out.

As my friend Dr. John Moir, Assistant Physician to the Edinburgh General Lying-in Hospital, has undertaken to send to your Journal an abstract of the cases which have occurred in that Hospital, since the year 1823, I shall take no other notice of Dr. Collins's observations respecting that hospital, than to point out a most deliberate misrepresentation respecting my account of that Institution, on which he has been pleased to found an impeachment of my veracity.

He says, page 419 of your last Journal, “Now for the

Professor's own Hospital, the Edinburgh. In the following statement, taken from Dr. Hamilton's third letter to the Editor of the London Medical Gazette, there is much of concealment.

“ ‘ It will, no doubt, surprise Dr. Collins and the gentlemen connected with the great establishment in Dublin, when I state, that by a Report presented to the Managers of the Edinburgh General Lying-in Hospital, and circulated under the authority of the Right Hon. the Lord Provost of this city, dated the 21st January, 1837, it appears, that 15,936 women had been delivered previous to 1st October, 1836, and that the whole expenditure, (not the annual,) including the purchase of the buildings and area, furnishing the same, &c. amounted to the very small sum of £10,214 13s. 8d.’

“ Would not the unsuspecting reader from this extract believe that the total 15,936 women were delivered in the Edinburgh Hospital; whereas the fact is far otherwise, as by a statement printed in 1834 (I have not that to 1836) the deliveries within the walls from 1793 to the former year, amounted only to 5198.”

This formal and very serious charge is rendered plausible by the interpolations of the words *in the Edinburgh Hospital*, and the words *within the walls*, and by the omission of one-half of a sentence.

In page 387 of the London Medical Gazette for June, 1837, I have stated, that “ it is well known also since the establishment of the Edinburgh General Lying-in Hospital, in 1793, I have had the chief charge of that institution; and although it is upon a scale quite inconsistent with the extent of our population, yet 15,936 patients were delivered by the medical attendants of the hospital, previous to 1st October, 1836.”

Dr. Collins has had the hardihood to declare his disbelief in this statement, and triumphantly brings forward a quotation from the annual address to the public in favour of the hospital, printed in 1834, to prove that only 5189 patients were delivered within the walls of the hospital. The printed sentence in

the address from which he has taken half a sentence, is in the following words: "5198 patients have been delivered in the hospital, and 9126 out patients have been attended at their own houses, at an expense not exceeding £9650, independent of the building and area, which are so admirably fitted, in point of quietness and ventilation, for an hospital of that description."

During the years 1835 and 1836, the total amount of patients was 1576, which being added to the former number of 14,360, make 15,936, the exact number which I have stated to have been attended by the medical officers of the institution. I shall leave your readers, Mr. Editor, to apply the epithet which conduct such as this so well merits. Forty-six years have elapsed since my father and I, in a letter to Dr. Osborne, stated the following incontrovertible axiom—

"By mutilating an author's expressions, and selecting particular passages without adding those which explain or elucidate the subject, any opinion may be misrepresented, and any meaning may be applied."—*Hamilton's Letters to Osborne*, page 31.

It is impossible to imagine a more correct verdict upon Dr. Collins's two articles in your Journal, than this sentence conveys.

He not only has interpolated words, and suppressed the half of a sentence, in order to pervert my meaning, and to give a colour to his uncandid allegations, but he has even accused me of concealment in respect to the affairs of that hospital, and yet in a note preceding the paragraph which he has quoted from my third letter to the Editor of the London Medical Gazette, I have referred him to a document, proving, by the publication of Mr. Moir, in 1823, that a daily report of the individual cases had been kept by myself for the first seven years, but that the books containing those reports had been abstracted.

In the paragraph succeeding that which he has quoted, it is

stated that a regular account of the cases which occurred from 1st January, 1823, to 31st December, 1836, and of the out patients of the same institution, from September 30th, 1825, to 31st December, 1836, has been kept, and an abstract of the result, in what relates to laborious labours, has been added. Nobody but Dr. Collins would venture to allege, that this is concealment; and when I add, that since the institution of the hospital, an annual printed account of the number of patients delivered, and of the very items of expenditure, has been distributed among the subscribers; and that upon an average, nearly one hundred pupils annually attend the hospital, at the expence of £1 3s. for six months' attendance, it would require even more dexterity than Dr. Collins gives me credit for, to conceal the affairs of the Edinburgh General Lying-in Hospital.

Having thus pointed out, that by means of interpolations and substitution of words, and by garbled quotations of sentences and paragraphs, Dr. Collins has contrived to render plausible his various misstatements and misrepresentations of my opinions and practice, I have only to add, that it is impossible for me ever to have any future communication with that individual.

I have the honour to remain, Mr. Editor,
Your obedient, humble Servant,

JAMES HAMILTON.

ART. IX.—*Observations on Fever.* By ROBERT LAW, A.M.,
M.D., Physician in Ordinary to Sir Patrick Dunn's Hospital.

ALTHOUGH there is no disease that has afforded more ample opportunity for the investigation of its nature, and of all circumstances connected with it, than fever; from the frequency of its occurrence both as an epidemic, and in isolated cases; from its sparing neither age nor sex; from being confined to no

particular climate or locality ; and from its ever having engrossed a large share of the attention both of the practical physician and of the pathologist : we still find there is no disease upon whose nature more obscurity prevails ; none in connexion with which there exist more disputable points, and these points most intimately and essentially affecting the very nature of the disease. Among the many causes which contribute to involve the nature of fever in doubt and uncertainty, we would advert to a few which appear to us especially to conduce to it. Ist. The imperfect state of both our anatomical and physiological knowledge of that portion of the animal economy, in the derangement of whose functions the earliest phenomena of fever exhibit themselves, we allude to the nervous system. From our incompetency to appreciate the first effects of the morbid principle of fever upon this system, and analogy seeming to point to another system with whose functions we are better acquainted, viz. the absorbent system, as the channel through which the matter of fever entered the body, a theory was framed to meet the state of our knowledge, attributing fever to the passage of a miasma or noxious subtile fluid into the economy through the absorbent vessels. This theory was countenanced by the fact, that when one exposes himself to the contagion of fever fasting, he is more likely to contract the disease than when in an opposite state. It was inferred from this, that this greater susceptibility arose from increased energy of the absorbent system ; that the vessels of this system, being refused their natural and wholesome nutriment, became as it were exasperated, and swallowed whatever came in their way, and thus took in this miasma or materies morbi, and conveyed it into the system. This is the most generally received theory of fever, and this is the explanation that most physicians would give of their practice of not exposing themselves to contagious fever when the stomach is empty. We quite agree in the wisdom of the precaution of satisfying the absorbents, but deny that they are more the channels through which the morbid

matter enters the system in this instance than they are in other cases where there is no reason to suppose either that they are in an unusual state of activity ; nor if they were, can we discover any contagion to serve as a *materies morbi* for them to exercise themselves upon. These are cases in which a strong moral impression acts as a direct and immediate cause in the production of a fever similar in all respects to one from contagion ; or when the body having been weakened either by anxiety, watching, or by intemperance, and then been exposed to fever, and become affected. This susceptibility in these instances is of the same nature as that resulting from fasting, and is the effect of the debility of the system, and not of the activity of the absorbents. The second cause to which we would attribute the obscurity connected with fever, is the little opportunity that a medical man has, in general, of seeing the disease in its earliest or nascent stage, his assistance being seldom sought for till the first symptoms have lost their simplicity, from being modified by, and mixed up with others which succeed, but which are no otherwise connected with the primitive symptoms, than as the several parts thus secondarily affected stand in sympathetic relation with those upon which the morbid impression is first made. Thus will it not unfrequently happen, that those secondary symptoms being much more striking and prominent in their character, will claim to themselves an exclusive attention, throwing, as it were, into the shade the primary ones, and causing them to be almost, if not altogether, overlooked. Thus do we find that all the symptoms of fever antecedent to the increased frequency of pulse and heat of skin, are taken no account of in the history of the disease ; while this latter symptom has been selected as so essential as to be the one to give a name to it, although in many of the most striking cases this feature is altogether absent ; and, at all events, in most does not come on until other, though perhaps less striking symptoms, have already existed. This obscuring antecedent by subsequent symptoms, is particularly to be observed when the inflammation of the intes-

tinal mucous glands complicates the original symptoms of fever. These secondary symptoms will, in fact, assume so serious a character, as to constitute the principal danger. The frequency of this complication has led to the opinion that it was the essence of fever ; whereas, we are confident, from repeated examinations of the bodies of individuals who have died of the disease in an early stage of it and exhibited no affection of these glands ; and of others, whose early symptoms differed only in being less intense, and who having passed this early stage, have fallen victims to the complication ; that the complication is accidental and not essential.

An intelligent writer on fever, in the warmth of his opposition to the doctrine of essential fever, or of fever independent of sensible organic lesion, while he is obliged to admit, that in the bodies of some who die of fever, no organic lesion can be detected, explains this unfavourable circumstance to his theory by *supposing* that had the subjects of these cases lived longer, the requisite lesion would have taken place ; that death was too expeditious for it ; in other words, that the disease not only preceded its cause, but actually reached its fatal termination before the tardy cause made its appearance. Into what strange inconsistencies, and even absurdities, will the support of our pet theories not unfrequently betray us !

In asserting the very frequent disease of the intestinal glands to be an accidental complication, and not essential to fever, we feel all the importance of the subject in a practical point of view ; nor would we dare to advance it, did we not feel ourselves warranted to make the assertion from the result not only of *post mortem* examinations, but from a close watching of symptoms as they developed themselves, manifestly exhibiting by cognizable signs the affection of the glands, and from this period stamping the disease with features which it hitherto had not.

Our city is now, and has been for some months past, more free from fever, than it has been for nearly three years ; typhoid

fever having prevailed the greater part of that time, with varying intensity, and under various modifications. Among these modifications we had the opportunity of abundantly witnessing that connected with disease of the intestinal glands ; this complication constituting the striking feature of the epidemic for a time. It is from the results of our experience of the epidemic when presenting this character, that we question its being essential, finding it, as we before stated, not present in the bodies of those who were quickly carried off by the disease ; nor discovering in an early stage of the disease, those symptoms which afterwards exhibited themselves, indicating the complication. These varying phases of fever, the result of accidental complication, have contributed their share to the difficulty of fixing the nature of fever, causing partial and secondary affections to be considered as primary and fundamental modes of derangement.

We shall proceed to detail some cases of fever, which seem to us calculated to throw some light upon the mode in which the first morbid impression is made upon the system in the production of the disease ; and see how far these cases tend to confirm the opinion that fever is the result of a miasma conveyed into the system by the absorbents : or if it be not, at least in some cases, the effect of a moral impression acting upon the nervous system, and exhibiting itself in symptoms indicating a derangement of the functions of this system.

CASE I.—John Gray, aged 30, grocer, admitted into hospital September 29th, 1836. A week since he was seized with a rigor, which he says, almost immediately followed upon what he calls a “ turn,” which he felt at hearing a woman scold her child for visiting a person ill in fever. Shortly afterwards he felt soreness in his head, eyes, and neck.

Present phenomena. Pulse 108, soft ; dry burning heat of skin ; elevated red spots on chest and abdomen. Face flushed ; eyes suffused and watering ; lips red. Tongue dry, and red at point and centre ; respiration hurried, and sighing. Thirst not

urgent; abdomen soft, not sore on pressure; expression peculiarly anxious.

℞ Mist. Camphoræ, ℥iv.

Aquæ Acetat. Ammoniæ, ℥ii.

Spirit. Ammon. Aromatic. ℥i.

M. St. ℥i. 3tiis horis.

℞ Camphoræ gr. ii.

Pulv. Jacobi gr. i.

Ft. Pilula ter die sumenda.

Body to be sponged with tepid vinegar and water.

30th. Pulse 108, wiry. Spots on chest and abdomen more developed. Belly tympanitic; tenderness in ilio-cæcal region; tongue red, dry, and florid at point; bowels not free; urine scanty. Had disturbed muttering sleep; has every mark of extreme nervousness.

Enema Terebinthinæ statim. Hirudines duodecem regioni Ilio-cæcali. Cataplasma Abdomini postea. Fove crura. Adde Mist. heri prescriptæ Aceti opii gutt. xx. Rept. Pil. Camphoræ et Pulv. Jacobi.

October 1st. Pulse 120, soft, weak; skin densely covered with elevated purple petechiæ, resembling a papular eruption; tongue dry, reddish-brown, glazed in centre. Abdomen tympanitic; countenance less anxious; a copious discharge of urine followed; friction of the abdomen with turpentine.

Repetr. Medicamenta.

2nd. Subdelirium through the night; pulse 108, feeble; is rather sleepy and drowsy; countenance tranquil and not anxious; bowels free; urine copious.

Repetr. Medicamenta. Lotio frigida fronti. Fomentationes Terebinthinatæ abdomini et cruribus.

3rd. Chest and abdomen thickly covered with dark purple petechiæ; face flushed, eyes suffused; tongue dry, rough, and brown in centre. He answers questions consistently, but very

slowly ; abdomen tympanitic, but not sore when pressed ; copious and involuntary discharge of urine. Bowels not free ; had delirium through the night.

Hirudines sex pone singulas aures. Lotio frigida fronti. Fomentationes Terebinthinatæ abdomini et cruribus. Enema Terebinthinatum.

4th. Face much less flushed ; eyes less suffused ; answers much more immediately ; pulse 114, of tolerable firmness ; belly tympanitic ; petechiæ become more of a brown colour ; tongue dry, rough, and of a whitish brown colour. He had no delirium through the night.

5th. He passed a considerable quantity of urine under him during the night, and this morning ; still hypogastrium very full. Catheter introduced, drew off nearly two quarts of dark, ammoniacal urine ; pulse 96, full ; skin soft and moist ; belly, which was tympanitic before introduction of catheter, now flaccid and pliable ; countenance composed and tranquil.

Mist. Cardiac.

6th. Pulse 96 ; skin soft and moist ; considerable tumefaction and fulness of hypogastrium, although a large quantity of urine has escaped from him in the bed. Catheter has again drawn off not less than two quarts of the same character of dark-coloured urine. Tongue dry and rough.

Haustus effervescens c. Spiritûs Ammon. Aromatic. gtts. xx. 3tiis horis.

8th. Has had no involuntary discharge of urine since last report. Required the instrument again to be introduced, which drew off three pints of water. All febrile symptoms almost gone. The bladder soon recovered its power ; and in a very few days from hence we were able to pronounce him convalescent.

The preceding case presents us with some interesting considerations. In the first place, it exhibits to us strikingly the

mysterious influence of a moral impression, not only in predisposing to, but in actually producing disease. The young person, who had been the subject of his mother's rebuke for visiting the individual in fever, did not himself contract the disease, nor was Gray exposed to fever from any other source, as far as we could learn. Another consideration suggested by the case is the necessity of closely attending to the state of the bladder in fever, and not to allow ourselves to be thrown off our guard by even a full discharge of urine, as we here see how compatible it is with a very loaded state of the organ. The involuntary discharge of the urine, under such circumstances, may be, (as we doubt not was the case here,) not the result of relaxation of the sphincter, nor of that mental absorption and preoccupation, which so often in fever allows the calls of nature to pass unnoticed; but the atony and partial paralysis, and consequent loss of control of the organ over its contents from its prolonged distention.

CASE II.—Eliza Jordan, aged 26; unmarried; of a nervous temperament; admitted into hospital March 28, 1836. She had been in perfect health a week since, when on missing a piece of linen which had been committed to her care, to make shirts, from the apprehension that her honesty would be called in question, she was seized with a violent rigor and sickness, which confined her to bed ever since. Present symptoms:—pulse 150, small; skin hot and moist; tongue covered on upper surface with whitish yellow fur, red at point and edges; respiration hurried and sighing; complete loss of sleep; she says she has not slept since commencement of her illness; bowels obstinately torpid. Has a disposition to weep; has a distressing choking sensation in the throat; complains of tired, fatigued feel; no headach, but feels her head light; thirst.

Enema Terebinthinæ statim.

R Mist. Cardiacæ ℥ i. 3tiis horis.

Haustus e Liquoris Opii Sedativi gtts. xv. h. s.

30th. Skin, especially on chest and back, thickly covered with livid purple petechiæ ; gums soft, spongy, and disposed to bleed ; pulse 132, soft ; tongue large, flabby, and not much coated. She slept well last night, and even still seems under the influence of opium.

℞ Mist. Camphoræ ℥ vi.

Spirit. Ammon. Aromatic. ℥ i.

M. Sumat. ℥ i. 3tiis horis.

31st. Pulse 150, small ; petechial eruption more general over the body ; face flushed ; tongue covered with yellowish fur ; she moans continually ; respiration laboured and sighing ; thirst very urgent ; slept none last night ; urine of a dark brown colour.

Mist. Cardiac.

Aqua c. Acid. Nitro Muriatico diluta, pro potu.

Haustus e Liquoris Opii Sedativi gtts. xii. h. s.

April 1. Pungent heat of skin ; face very much flushed ; pulse extremely small, scarcely can be counted ; tongue dry and brown in centre, red at point and edges ; abdomen very tympanitic ; epigastrium sore when pressed ; dorsal decubitus, with legs drawn up ; insatiable thirst ; skin very much mottled ; was very restless during the night, and could with difficulty be kept in bed ; headach.

Hirudines duodecem epigastrio. Cataplasma amplum abdomini postea. Lotio frigida fronti. Foveantur crura. Vini ℥ viii. Ene-
ma Terebinthinæ.

2nd. Pulse 144, very small ; skin less hot, and less mottled ; moans continually ; tongue dry, brown, and rough ; bowels obstinately torpid. Is quite conscious.

℞ Olii Ricini.

Olei Terebinthinæ.

Aqua Menth. Sing. ℥ ss.

Fiat haustus statim sumendus. Repetr. Vinum.

3rd. Pulse 120, stronger ; bowels free ; is heavy and stupid, but perfectly coherent.

Vesicat. Nuchæ. Foveantur crura.

4th. Pulse 120, small. Is still in a dead, heavy state ; involuntary and copious discharge of urine ; she moans incessantly ; deglutition difficult ; abdomen tympanitic, and sore on pressure ; bowels confined.

Vesicat. vertici. Enema Terebinthinæ. Foveatur abdomen. Wine whey.

5th. Pulse 126, stronger ; face flushed ; eyes suffused ; abdomen not sore when pressed.

White wine whey.

6th. Sixteenth day of fever. Pulse 114, small, but distinct ; duskiness of skin much less ; face less flushed ; countenance improved ; abdomen flaccid ; tongue still dry, and brown at point and centre, moist at edges. Expresses a wish for flummery.

8th. Pulse 96 ; tongue still dry, and brown ; much improved otherwise. Menstrual discharge present.

10th. She continues to improve.

12th. Febrile symptoms all gone. Convalescence was complete, but slow. The mind took at least as much time to recover its energy as the body.

The direct and immediate effect of a moral cause in the production of fever, is no less apparent in this than in the preceding case. The nervous character of the fever is, however, more strongly marked in this than in the other. We find it commencing with rigor, which so commonly attends injurious impressions on the nervous system, such as a shock, fright, &c. The other symptoms also equally indicated how deeply the nervous system was involved. The soft, spongy condition of the gums, with the disposition to bleed, connected with the exten-

sive livid petechial efflorescence, gave the case a character not very unlike purpura hæmorrhagica, or scurvy; and tended to confirm us in an opinion to which we had been led by other similar cases as to the rationale of these dark-purple petechiæ, which we conceive we have observed most frequently, and most markedly, when the nervous system is most deeply engaged. We know the effect of lightning, and other causes which destroy life by a strong impression on the nervous system, such as a blow on the stomach, is to deprive the blood of its power of coagulation, and, as it were, to break it up and dissolve it. So, in these fevers, the blood becomes attenuated by its loss of vitality, and passes into the capillaries, whose diminished tone, from the general debility, passively contributes to the same effect. How often do we meet passive hæmorrhage attacking individuals who have suffered from depressing moral causes?

CASE III.—Mary Keville, aged 27; married; admitted into hospital May the 3rd. She complains of a most distressing pain in the epigastrium and left hypocondrium. Pulse 126, soft and feeble; skin hot and dusky; tongue covered with a drab-coloured fur; abdomen soft, not painful on pressure. She has had no sleep for several nights. Great prostration of strength and depression of spirits. The account she gives of her illness is, that she came in perfect health to see her husband who was ill of fever in the hospital; she had not been long in the ward, when another patient, in a state of delirium, jumped out of bed and ran towards her; as she attempted to escape from the ward, she fell, and struck her left side against the door; she immediately felt sick and unable to walk. Soon afterwards she was seized with the pain, or what she describes as a burning sensation in the epigastrium and hypocondrium, which from the time of its first coming on a week ago, till now, has never left her. All this time she has been confined to bed.

Adhibeantur Hirudines duodecem regioni dolenti. Haustus, e Liqueoris Opii Sedativi gutts. quindecim, et Mist. Camphoræ: uncia, horâ somni sumendus.

4th. The pain has completely yielded to the leeches. She has had some hours of refreshing sleep. Tongue covered with greenish-yellow fur. Pulse 132, extremely feeble and compressible.

Three ounces of wine.

5th. Complains of heat of stomach. Pulse 138, small and thready. Pressure excites no pain in any part of the abdomen, except in the ilio-cæcal region; and there it is very inconsiderable. No tympanitis. Skin generally spotted. She moans continually.

Wine, three ounces. Cataplasma abdomini.

8th. No amendment in symptoms since the last report. Pulse 138, extremely feeble. Tongue, lips, and teeth, covered with a blackish incrustation; great restlessness; abdomen not tympanitic; skin spotted, rough, and furfuraceous; thirst very urgent.

Wine eight ounces. Mist. Cardiac.

9th. Pulse very feeble. Hands and nose cold. Tympanitis in a slight degree; surface thickly covered with dark petechiæ; dense fuliginous incrustation of tongue, lips, and teeth; face flushed; eyes suffused. She had no rest through the night.

Twelve ounces of wine.

10th. Pulse 138, a degree stronger. She has lost the almost cadaveric coldness which she had yesterday. Abdomen more tympanitic; face flushed; is quite coherent.

Eight ounces of wine. Enema Terebinthinæ.

11th. Pulse 108, small and thready. Hands and tip of nose have again become quite cold; prostration extreme; consciousness perfect.

Twelve ounces of wine, or more if necessary.

12th. Pulse 102, with some degree of strength. Tempera-

ture of hands and nose natural. Cuticle desquamating. Debility very great.

Repeat the wine. Beef tea.

13th. Face flushed ; tongue dry and incrustated.

15th. Pulse 108, much firmer. Tongue continues dry. She is in every other respect much improved.

Four ounces of wine. Beef tea.

16th. Pulse 90. Although the tongue continues coated, her appetite has returned.

17th. Tongue quite clean. The skin of almost the entire body has desquamated, as if she had had erysipelas or scarlatina.

22nd. Is quite convalescent. Her strength returned very slowly.

How are we to explain the mode of this individual's attack of fever. If we are to suppose it was contracted from exposure to contagion, we would avail ourselves of the argument of the toxicologist, who reasons that from the very short period of time in which some poisons exhibit themselves in the system, these poisons must affect the system through the medium of the nerves rather than through the circuitous route of the circulation. She was satisfied that her inability to walk, and immediate sickness, were not the effect of her falling against the door, but of the fright. Nor can we believe that the fever was symptomatic of an injury inflicted by the fall, as we cannot conceive that any local injury can give rise to a fever resembling petechial typhus fever in all its symptoms. Although the type of fever attending injury may be very low and asthenic, we believe petechiæ will never be present in such asthenic symptomatic fevers.

CASE IV. ——— Farrington, aged 36 ; married ; nurse of the male chronic wards in Sir Patrick Dunn's Hospital, admitted into fever ward, October 12, 1836. She had been ill for several days before she came under our care ; she had tried to fight against her illness, but was at length obliged to give way. The history of her illness was this : A man was brought into the

hospital affected with delirium tremens, so violent that he broke the porter's arm, when he attempted to put the strait-waistcoat upon him. He was at length secured, and placed in a separate apartment in the lower part of the hospital. His feat of violence had rendered him an object of curiosity; and the nurse must needs see the wild man. When she saw him he had such an expression of savage wildness, that she was quite terrified; she could scarcely stand; felt herself covered with cold perspiration, and was quite unable to walk up stairs. Her stomach then became sick, and she was seized with violent headach. Leeches applied to temples relieved the head.

She had been five days confined to bed when she was removed to our fever ward, and presented the following symptoms. Pulse 108, small. Face flushed; eyes suffused; tongue moist at point and edges, red and dry in centre. She complains much of dryness of tongue and throat. Skin dusky, hot and dry; abdomen tympanitic, not tender when pressed. Muttering delirium through sleep. No intolerance of light.

Abradtr. Capillitium. Lotio frigida fronti. Foveantur crura.

℞ Mist. Camphoræ ℥ iv.

Aqua Acetatis Ammoniae ℥ iii.

Spirit Ammon. Aromatic. ℥ i.

Syrupi Croci. ℥ i.

Misce. Sumat Cochlear. duo ampla tertiis horis.

22nd. She seems much better. Her countenance indicates much less anxiety; face less flushed; eyes less suffused; has now scarcely any headach. She had quiet, tranquil sleep through the night.

24th. Pulse 108, soft. Skin cooler and less dusky. She complains of slight headach, and had some delirium through the night.

Lotio frigida fronti. Foveantur crura. Adde Mist. antea prescriptæ Aceti Opii gutts. viginti.

25th. Was quite delirious through the night. Pulse 96,

weak and compressible. Face flushed ; eyes heavy ; skin rather cooler ; tongue moist and covered with whitish slimy mucus. She has had a nervous anxiety and great depression of spirits since the commencement of her illness.

Hirudines quatuor pone singulas aures. Lotio frigida fronti: Fomentationes Terebinthinatæ cruribus. Repr. Mist.

Two days from this she improved so much that we were soon able to pronounce her convalescent.

Although the subject of this case, being an inmate of the hospital, was always as much exposed to the infection of fever as she was at the time of her seizure, it was only now when she was frightened that she became affected. She had been in perfect health before the attack ; nor was the case of delirium tremens, one of fever in which the delirium tremens was accidental, but one of pure delirium tremens, and therefore, not one to communicate infection. A degree of nervous excitement marked her illness, through its entire course.

CASE V.—Mrs. Dyas, aged 26, admitted into hospital 20th May, 1836, presenting the following symptoms : pulse 138, full and firm ; skin universally covered with an efflorescence scarcely to be distinguished from measles ; lips and mucous membrane of mouth of a deep red colour ; a peculiar brilliancy of eye ; tongue clean and red ; abdomen soft, not tender ; thirst moderate. She states that she has been ill for six days : that she had been in close attendance on her brother, who was ill of fever, and had not spoken for two days, when he shouted, and gave her a sudden start ; she immediately felt a pain in her back and head, with sickness of stomach and general weakness.

Haustus effervescens c. Spiritû Ammoniac Aromatic. Pulv. Jacobi veri gr. duo. quartis horis.

21st. Skin more thickly marked, and efflorescence of a more crimson hue. Complains of fulness in her head, or rather corresponding to the frontal sinuses. Diarrhœa.

Pulv. Doveri gr. quatuor, quartis horis.

23rd. Pulse 132. She complains of distressing tinnitus aurium. Bowels still relaxed; efflorescence of a darker colour; face flushed; eyes watery; lips red; tongue clean.

24th. Skin does not exhibit the same congested appearance; a few dark, circumscribed spots on chest and abdomen. She has never felt very ill at any period of her sickness. On the 28th, we marked her convalescent.

Although the subject of the preceding case was under circumstances of exposure to the matter of infection, so that it could not be a cause of wonder her contracting the disease; still the mode of seizure was sufficiently remarkable, and of such a nature as to seem to warrant its being placed along with the cases which it so closely resembled in the share which a moral impression had in its production. It is not, in general, when one is in actual attendance on a sick friend in a contagious disease, that illness is contracted: susceptibility seems to be suspended till the mind is somewhat relieved of its anxiety, and then, when the improved condition of our friend ceases so deeply to engage our sympathies, the danger to ourselves begins.

CASE VI.—Eliza Brereton, aged 19; unmarried. Admitted into hospital, March 5th. She was unable to give an account of herself. Her mother states that eleven days since she received brutal treatment from a person in whose employment she was; that she was immediately seized with violent headach, and complete inability to walk home. The next day her face broke out in large blisters, which soon disappeared. She then was affected with violent pain of the head and throbbing of the temples, for which she applied leeches, but without the least relief. She complains at present that she never closes her eyes in sleep. Pulse 140, full and corded; respiration very hurried and panting; expression very anxious; tongue loaded with yellowish fur. She complains of a hammering sensation in her head which distresses her greatly. Prostration of strength and depression of spirits extreme; thirst very urgent; face and surface generally exhibit a dead pale, exanguious,

hue, like one who had sustained a profuse hemorrhage. She was so deaf that we found it no easy matter to procure answers to our questions.

* Mist. Cardiac. Vini ζ iii. Fove crura. Haust e. Liquoris Opii Sedativi gtts. xv. h. s.

6th. Pulse 140, wiry; she complains much of noise in her head; had some broken sleep; expression very anxious; tongue cleaner, and more moist.

Repetantur Medicamenta.

7th. Pulse weaker and less wiry; had some quiet sleep during the night; she moans continually; abdomen not tympanitic, nor sore on pressure. Her symptoms underwent no change till the 10th, when the respiration became more laboured, the distressing sensation in her head more urgent, and her symptoms generally indicated approaching dissolution, which took place next day. Examination of the body exhibited no other morbid appearance, than a very scanty serous effusion on the surface of the brain and into the ventricles, with, perhaps, more red dots than usual on making a horizontal section of the organ. The heart was soft and flabby in its structure.

How are we to designate the preceding case? Neither its symptoms during life, nor the appearances after death, entitle it to be regarded as fever. We would adduce it as an illustration of the prompt influence of moral causes, stifling, as it were, the principle of life, without producing, at least, any sensible change in organization. Many of the symptoms reminded us of the effects of a profuse hemorrhage, which exhibit themselves especially in the derangement of the nervous system. The case derives additional interest from its connexion with one which we are about to detail.

* R Mist. Camphor. ζ vii. Carbonat. Ammoniae \mathfrak{D} ii. Liquor. Hoffman. ζ ii. Syrupi Croci. ζ vi. M.

CASE VII.—Mary Birmingham, a strong healthy person both in mind and body, nurse in the fever ward, describes herself, as becoming suddenly ill, when in raising the subject of the preceding case in bed, she felt herself, as she says, grasped by the cold, clammy hand of death. She had been quite well before, nor was there much fever, or of a bad type in her ward. On the third day of her illness, the body, especially the chest and abdomen, were covered with a diffused, measly efflorescence; her countenance exhibited a stupid, intoxicated expression; depression of spirits and prostration of strength were extreme; tongue large and flabby, and covered with yellowish, slimy mucus. She complained of a most disagreeable taste in her mouth. Leeches applied to the nape of the neck relieved a most distressing pain in the back of her head. She presented the following symptoms on May 18th, (the twelfth day of her illness,) pulse 108, full, soft, and compressible; petechiæ fading; occasional irregular action of the muscles of the left hand and arm; tongue covered with whitish yellow fur; no headach; no abdominal tenderness; great depression of spirits, and prostration of strength.

21st. Pulse 90, soft, and very compressible; she complains of soreness when epigastrium is pressed; disagreeable fetor of breath; face flushed; subsultus tendinum, and irregular action of hands; prostration extreme.

Arrow root with a small quantity of Brandy.

R Moschi ʒss.

Aquæ Menthæ Piperitid. ʒv.

Liquor. Hoffman. ʒii.

Syrupi Croci ʒvi.

Misce. St. ʒi. tertiis horis.

23rd. Pulse stronger; no subsultus; expresses a desire to eat something.

Beef tea. Four ounces of wine.

25th. Very much improved in all respects.

She was soon declared convalescent, but was a long time recovering her strength.

What inference are we to draw from the preceding case? The febrile seizure followed almost immediately after she had been employed about Brereton. At the time there was not a bad fever in her wards, nor was she aware of any thing that could have rendered her more susceptible of febrile infection, (had she been exposed to it,) than at other times. We can only regard the mode of attack, as of the same nature with others, where the absence of contagion, as the *materies morbi*, was more unequivocal; since the suddenness of the seizure, and the nature and character of the subsequent symptoms established an analogy between them and it.

We shall enter into no elaborate comment on the preceding cases. The object with which we have recorded them, is to try to prove from the nature of the cause of fever and the mode of its impression upon the system, as well as from the first and earliest symptoms, that it is the nervous system that is mainly interested. The cause which seemed to operate in all the cases which we have adduced was a moral cause. And, although in some of the cases it would have been difficult to have proved the absence of infection, where it might have been supposed only to have acted as a predisposing cause; yet in others no infection could be suspected, and therefore we must regard it as the direct and immediate cause; and as there was no difference in the symptoms of the cases when the infection was more questionable and of the others, it is reasonable to suppose that the mode of the impression was the same in both. We observed the same nervous character to pervade both. As in those cases where there was no reason to suspect infection, there was no palpable *materies morbi* for the absorbents to exercise themselves upon, and in those where infection might possibly have operated, (although the circumstances rendered it very improbable,) neither the suddenness of the effects after the exposure, nor the nature of these effects, constituted any analogy between them and

cases in which the principle of disease acts confessedly upon the system through the absorbents, we see sufficient reason to question these vessels being the channels through which, whatever it be that causes fever, produces its injurious effects upon the system.

Indeed we might say, that even in cases where there was most reason to suspect absorption, when a person having exposed himself to contagion, fasting, and then contracted the disease; even here, the symptoms exhibited by the disease, so resemble those, where there is no possibility of suspecting infection, that we cannot but believe, that the mode of impression is the same in both cases, and, that as it is not absorption in the one case, neither is it in the other.

As to the precise mode in which the impression is made upon the nervous system in fever we know too little, either of the morbid principle or of the system upon which the impression is made, even to attempt a solution. We can only say, that the first cognizable effects are observed in the nervous system. How frequently is rigor (which may be regarded as a nervous symptom, whether occurring as an irregularity of the calorific function, or as the effects of a moral impression,) the first announcement of fever? Have we not the organs of sense almost immediately responding to the morbid impression; and how soon follow the complaint of the pain in the back, and the mis-called pains in the bones, but really of the nerves proceeding from the spine and distributed to the different parts of the body? Then have we in their order the derangement of the functions of the nerves which preside over organic life, viz.:—deranged appetite, derangement of the secretions, &c.; add to these the more or less complete prostration of strength and depression of spirits, the almost constant accompaniment of fever. We would now ask, what system in the economy, or what part of organization is there, whose affection is enough to account for all these? We unhesitatingly reply, none but the nervous system.

ART. X.—*On the Treatment of Hydrocele, by the Injection of Tincture of Iodine into the Tunica Vaginalis.* By Dr. F. W. OPPENHEIM.

[Translated from the Zeitschrift, f. d. Gesamnte Medicin, by S. L. L. BIGGER.]

PERHAPS in no point of surgery is there to be found greater differences in Germany and in other countries, viz. England, France and Italy, than in the treatment of hydrocele. From the time of Celsus up to the present, the attempts at cure, and the manipulations to get rid of the dropsical collection, have been varied and altered in an extraordinary manner. Lately, accupuncture has respectively and successfully been employed in England, by Lewis, King, and Travers; simple puncturation, or puncturation with subsequent injection of various fluids, by Monro, Lambert, Richerand; either the fluid which has been drawn off, milk, water, hot, warm and cold, port wine, (Earle); either pure or diluted, and of various temperatures, camphorated spirits, solution of sulph. zinci, kali, carbon, kali caustic, argent nitricum, inflation of air, (Gimbernat, Schreger); laying the tumour open, (Hunter); cutting out a portion of the tunica vaginalis, (Celsus, Paulus, Æginetus, Douglas, Richter, Kern, Graefe, &c.); scarification of its inner surface, application of caustics, (Guido von Chauliac, Else, Dussansoy); actual cautery, (Paulus Æginetus); the application of pledgets, (Franko, Theden, Warner); of little canulæ, (Alexander Monro, Larrey); the seton, (employed by Galen, Lanfranchi, in the middle ages, and in latter times, by Pott, Monro, Earle, Charles Bell, Langenbeck, &c.). All these means have been alternately employed and severally lauded, but for the greatest part again set aside. Taken collectively, they may be considered as mere modifications of two principal methods, the laying open by incision, and puncture. By the former, the testicle is brought into view after the water is evacuated, and we are enabled to judge of its condition; it is not so by the latter. The laying

open of the tunica vaginalis is the method which has been principally practised in Germany to obtain a radical cure ; and it possesses beyond the advantage, before mentioned, of bringing the testicle into view, and enabling the surgeon to determine from its condition whether its removal would be proper, or to set his patient's mind at ease with regard to it, still this advantage, that it promises a more certain means of cure than any of the others, and is a better guarantee against relapse. The only objections are, that it is painful, and, therefore, dreaded by patients who fear the knife ; it is slow in healing, and it may be rendered tedious by the formation of abscesses, by gangrene and sloughing of the tunica vaginalis. The other, puncturation, and the subsequent injection of any irritating fluid into the tunica vaginalis, which has been so generally employed in England, France, and Italy, is accompanied with less pain ; there are no cutting instruments used, the patient sees neither wounds nor blood, and entertains hopes of a speedy recovery. It has, however, the disadvantage of not giving us any information with regard to the state of the testis ; and the degree of inflammation which may succeed, can never be judged of beforehand ; sometimes being too slight, and thereby failing of producing a radical cure ; and sometimes too violent, and causing all the evils which could occur from incision, only in a higher degree, viz. formation of abscesses, gangrenous destruction of the coverings of the testis, violent inflammation which may extend into the cavity of the abdomen, &c. without considering the evil consequences arising from infiltration into the cellular tissue. It cannot be denied, that in private practice, a method of curing hydrocele is highly desirable, which would promise as sure a means of recovery as that by incision, without causing the patient so much pain and loss of time.

The observations and experiments of Martin, recorded in the seventh vol. of “ The Transactions of the Medical and Physical Society of Calcutta,” on injections into the tunica vaginalis of weak solutions of tincture of iodine, after puncturing and

evacuating the contained fluid, observations which are confirmed by other surgeons there, from their own experiments, speak very much in favour of this means. He employed the injection of iodine in more than two hundred cases with success. The advantages which injections with iodine possess over those with wine consist principally in this, that it is not necessary to perfectly fill the cavity of the tunica vaginalis, and thence there is less danger of any extravasation of the injected fluid into the cellular tissue of the scrotum; and as the iodine is a very easily absorbable substance, even did infiltration occur, it would not be as likely to cause a gangrenous inflammation as wine. Besides it is not necessary that the whole of the injected fluid should again be evacuated; many drachms, nay, even the whole of the fluid injected may remain, it will be absorbed, and produce no obstruction to a perfect cure. The pain is more or less violent but does not continue so long as after the wine injection; on the day but one after, those who have been operated upon can go about; and if it be necessary, may do so a few hours after the operation. The cure is effected in a much shorter time, (in three or four days, whilst with the wine injection ordinarily fourteen days are necessary;) after-treatment is not required, and relapse is very rare.

These, with the advantages I have mentioned before, determined me to undertake this means of operation in the cases which might occur to me. Hitherto I have had only fifteen opportunities of so doing, and as I have reason to be well satisfied with the results, I shall in the following pages communicate those cases, in order to excite further attention to them, and imitation of them, which have remained sufficiently long after the operation under observation to enable me to speak of the results with certainty.

CASE I.—Wüppe, a phlegmatic, torpid peasant, about 60 years old, had a hydrocele of the left tunica vaginalis for many years, the commencement and cause for which he could not tell

with any certainty. He had frequently been operated on by tapping by many different surgeons, and the last time by me in the beginning of July, 1836. In September, the tumour had attained its former size, that of a stout fist, and I then determined to use an injection of iodine. Six ounces of a citron-coloured fluid were evacuated through the puncture; and then the greatest part of a solution of one drachm of the tincture of iodine in three of water was injected into the tunica vaginalis. The pain was very slight, and on that account I rubbed the scrotum repeatedly in order to bring the injected fluid into contact with the entire sac of the tunica vaginalis. The testis felt to be somewhat enlarged, the epididymis tumefied. On the following day, the patient complained very little of pain. On the 29th, the scrotum was a little swollen and painful on pressure: on the 4th October, the old fellow met me in the street, carrying his basket, merry, and full of spirits; on the 10th October, I examined him and found the testis on the side operated upon somewhat larger than the other, also the epididymis enlarged, but there existed no trace of fluctuation. I have seen this patient since repeatedly, and he assured me on inquiry that he was perfectly cured. As he has neither appeared before me lately, nor before those surgeons to whom he formerly had recourse, I suppose that he has remained without recurrence of the disease. After the former tapplings, the collection of water often returned in the first eight days.

CASE II.—Mr. S ———, merchant, of sensitive temperament, in his 40th year, got in the beginning of the year 1385, without any known cause, a hydrocele of the left side of the size of an egg. I tapped it in December. In October, 1836, the disease had acquired its former size; it was tapped again, and surrounded with strips of sticking plaister during fourteen days. In April, 1337, the disease was again there. On the 29th, I tapped it again, and immediately injected 3ii. of the tincture of iodine, dissolved in 3vi. of distilled water, and allowed this

fluid to remain in the tunica vaginalis until severe pain occurred in the course of the cord. The pain continued violent till evening, and then ceased.

30th. Considerable swelling and pain in the scrotum.

May 2nd. Pain and swelling diminished; the anxious patient remained two days in bed, but got up of his own accord on the third.

3rd. The patient is able to walk from his country house to the city and back again without uneasiness. There is still some swelling, and a little less pain on handling. After three weeks every trace of disease was gone. Fourteen months after the operation, Mr. S—— continued still perfectly well.

CASE III.—Mr. W ——, merchant, ætat 42, of dark, melancholy temperament, subject to hæmorrhoids, suffered from hydrocele of both right and left tunica vaginales. The disease had occurred nine years before when on a journey, soon after leaping out of a carriage; but at that time it only engaged the left side. When the tumour, in the course of three months, had attained the size of a large pear, he went to a surgeon, who tapped it: two months later the operation had to be repeated. The patient then made a journey to the south of France, and when the fluid had again collected, was operated upon by Professor Delpech, at Montpellier, by tapping and wine injection; violent pain succeeded to this, with swelling of the scrotum. The patient remained lying in bed for fourteen days, but soon after that was obliged to leave Montpellier, and after the interval of some months, the disease had again returned, yet not to so large a size as usual. As this annoyed him less, he bore it for a longer period than formerly, but he still was obliged to have it tapped twice a year. Altogether it was performed twelve times: the last of which was by me in January, 1837. Shortly after that the patient made a journey of business, and when I saw him again in the month of May, the swelling had not only acquired its former size, but also the right side of the scrotum was

swollen almost to the size of a hen's egg. I now mentioned to the patient the iodine injection, and he assented.

On the 24th May I first punctured the smaller swelling on the right side, and immediately after, that of the left; from the former I evacuated two, and out of the latter four ounces of a slightly coloured pellucid fluid; 3ii. of tinct. iodinii, dissolved in 3vi. of water, were now injected by the canula into the tunica vaginalis of the left testis; after a few seconds' delay, I allowed the fluid to return into the caoutchouc bottle, and injected it in again, and proceeded in this manner till the patient complained of pain in the course of the cord and of the spermatic vessels. I then permitted the greater portion of the fluid to return into the bottle, drew out the canula, and covered the wound with adhesive plaister; the patient had sat upon the bed during the operation, and now laid himself in it. The pain was tolerably violent; I advised him if he felt it too violent to make cold applications to the part.

25th. The pain continued very violent for three hours, and then almost entirely ceased; after that there were no cold applications made. He passed the night quietly and free from fever. The scrotum very much swollen and red, doughy, and somewhat crepitating to the feel. He got up.

26th and 27th. Patient in same condition; the tumour felt a little softer, half fluid, and seemed formed of plastic lymph.

28th. Tumour decreased; patient resumed his occupation.

June 2nd. Tumour as before, inflammation has entirely disappeared; it is only on pressure that there is some irritation perceptible.

I did not see my patient now for a long time, and I hoped that the reaction might be sufficient to produce also in the tunica vaginalis of the right testis a degree of inflammation sufficient to produce adhesion. After two and a half months, on the 14th of August my patient appeared again. The left testis had nearly returned to its normal condition, only that it was a little harder to the touch than hitherto, a symptom which

indicated that the operation had perfectly succeeded, because this hardness shewed, that perfect adhesion had taken place between the testis and tunica vaginalis, and therefore that all interval existing between them was obliterated ; but the right had increased very much in magnitude, and resembled in form and size a large lemon. The patient told me that six weeks after the last operation every trace of disease had disappeared, but that from that time, without any known cause, the right side began again to increase, and that now it had attained its present troublesome volume. He desired that the operation which had been performed ten weeks before on the opposite side, should now as soon as possible be performed on this side too. There were difficulties in the way of the operation being performed in his own dwelling, consequently I performed it in a room in my own house. Seven ounces two drachms of clear fluid was evacuated through the puncture, and only a single ounce of the injection thrown in ; after ten minutes, when the pain in the course of the cord had become very great, the injection was withdrawn, yet so as to leave more than a drachm behind in the tunica vaginalis. The patient remained lying on a sofa with recommendations to keep himself quiet until I should return ; it was nine o'clock in the morning. When I came home at half past one my patient had disappeared, and I did not see him until four days afterwards ; he had walked three miles without any difficulty. The pain after the operation was not very great, so that after remaining quiet for two hours he got into a carriage and drove to the country. It was now and again after the operation that he felt a throb, which, however, only lasted a short time. The scrotum was still very much enlarged, (larger than a goose egg,) but the patient satisfied with his condition and confidently hoping for a cure was in good spirits. He now shewed himself to me every eight to fourteen days, in which periods of time in proportion to the disappearance of the inflammation, a remarkable decrease of the tumour occurred, and between the sixth and seventh

week it had entirely disappeared. I saw this gentleman to-day, (June 26th, 1838,) a year after one, and eight months after the other operation. He is in perfect health.

CASE IV.—Carl Winter, a hatter, æt. 33, for eight months had a tumour of the left tunica vaginalis. When he first remarked it, it was about the size of a small egg. He knew of no cause to attribute it to: for ten years he had not had any venereal affection. Latterly, the size of this tumour had increased quickly and remarkably; the right side was also somewhat swollen. The circumference of the left exceeded that of a large goose egg.

On the 20th of June, I punctured it, and evacuated nearly six ounces of a straw-coloured fluid; the testicle seemed to be healthy. I injected immediately 3ii. of tincture of iodine, with 3vi. of water, moderately warm, by means of the caoutchouc bottle of Bell, moved the scrotum up and down in order to bring the fluid into contact with the entire internal surface of the tunica vaginalis, and caused the fluid after to return into the bottle by relaxing the pressure made on it, and then injected it afresh, and finally, after some minutes withdrew it, when the patient complained of pain in the course of the cord and in the region of the loins. In the next two or three hours the pain was violent and extended to the region of the kidneys. However, the patient had no fever in the evening, and slept well during the night. Next morning there was no pain except on moving the scrotum, which was swelled to twice its original bulk.

25th. The tumour as large as before, but it is less irritable on motion, less tense, hotter, a kind of crepitation is to be felt, accompanied with a noise like the crumpling of gold paper.

30th. Swelling decreasing; free from pain.

July 10th. The patient has now very slight tumefaction.

CASE V.—Gl., æt. 71, a furrier and hair dresser, came to me in October 1837, with a hydrocele of the tunica vaginalis of the right testis of immense bulk, and expressed a wish that

I should operate upon it. He informed me, that he had become affected with the disease for the first time nineteen years before, when he caught cold after some violent exertion; he had been operated upon several times, but the disease had constantly returned, finally, a surgeon had promised to cure him perfectly, and had attempted to do so, by passing a strong needle and thread; a seton through the tumour of the left tunica vaginalis, which had been allowed to remain a long time caused him violent pain, and a considerable discharge of pus; he was obliged to keep his bed two months, and was even longer incapable of pursuing his occupation. At last, he was freed from the disease on the one side, yet the violent pain he had suffered, the length of time, and the expense which he had undergone, determined him rather to endure the disease of the right side his whole life through than submit himself again to so atrocious a method of cure.

The easy and quick recovery of his nephew, Carl Winter, (the preceding case,) determined him, however, to permit himself to be treated after the same manner, and he pressed me now anxiously to perform the operation for him. On account of the very advanced age of this patient I felt very little disposed to do so. I feared that the operation would not better his condition, that either on account of the great thickness of the tunica vaginalis it would be a failure, or that if successful nature might determine this aqueous secretion to which he was so long accustomed to, to some more important and nobler part, and thus cause ascites or hydro-thorax. The old man, however, would not yield; he was stout for his time of life, his general health was good, there was no trace of any disease in either chest or abdomen, yet previous to the hydrocele appearing he had suffered from asthmatic attacks. The tunica vaginalis felt, as might be expected, very thick. At length I yielded to the entreaties of the old man.

On the 4th of August I punctured and evacuated a large quantity of a dark yellow fluid, which was not measured. The

tunica vaginalis was excessively thickened ; the testis appeared small and atrophied when felt through it : I injected a solution of ʒiii. of tinct. of iodine in ʒiss. of water, and as after ten minutes the patient did not complain of much pain in the course of the cord, I drew out the canula and permitted the greater part of the fluid to remain. He walked home ; I saw him next four days after ; no reaction had taken place, but the old disease was again in progress. I was satisfied that there was no more unfavourable result, and did not allow myself to be prevailed on by the repeated entreaties of the old man to repeat the operation.

CASE VI.—Mr. M., a statuary, æt. 35, of choleric temperament, had for four years a small hydrocele of the tunica vaginalis of the left side, which he supposed to have arisen from a squeeze and pressure. It had been tapped three times by different surgeons in the city ; on the 7th October he came to me. The swelling was not so large as a hen's egg, and the testis was to be felt both below and behind. I evacuated through the puncture about one ounce of fluid. The testis and epididymis felt perfectly natural. The patient on this and the following days attended to his business in perfect health.

On the 12th of October he met with a violent blow on the testis, to which violent pain in the tumour succeeded immediately. I saw him on the evening of this day. The scrotum was swollen to double its former size, of a bluish-red colour, was more round than pear-shaped ; it was hot, painful on motion, not fluctuating, but almost solid to the touch and very hard. I diagnosticated inflammation of the testis to exist, perhaps complicated with extravasation of blood into the cellular tissue of the scrotum, or into the tunica vaginalis ; prescribed twelve leeches. On the next morning the pain was much less, the tumour was unchanged in size, but it was less hard and less tense to the touch. Ordered an emetic and dry fomentations.

14th. October. Pain of all kinds is less.

15th. I tried compression with strips of adhesive plaister.

19th. Indistinct fluctuation was to be felt.

20th. I punctured and evacuated a dark-reddish, brown-coloured fluid, with some flakes and coagula, the whole mass amounting to two ounces, three drachms. After the manner so often described, two drachms of tinct. iodine, dissolved in six drachms of water, were injected. For four hours there was violent pain. Next day felt himself well; tumefaction moderate.

22nd. Again attended to his occupation. Three weeks after the operation every trace of disease had disappeared. By the end of June 1838, the patient was in perfect health. The disease had entirely disappeared. Since this time he has had a rheumatic fever, and has recovered from a fall and violent contusion of his side.

CASE VII.—Mr. C., of the Island of Cuba, æt. 30, a very nervous, excitable, and anxious person, put himself under my care in July 1837. For six months he had a slight swelling of the right testis, which caused him much uneasiness. I did not feel quite sure of my diagnosis, as many criteria were wanting on account of the small size of the tumour. A light held behind it did not shine through the tumour; the testis could not be felt drawn upwards and backwards, but it was placed in the middle. This gentleman was so sensitive that he shrunk from very slight pressure, and complained of pain. I thought that I had to do with a tumour of the testis from the history of the case, and therefore I tried the effects of compression by means of strips of adhesive plaister, which caused him only slight pain; after this application had been made in vain for fourteen days, it seemed to me more probable that it must be a hydrocele, and an acupuncture needle prudently introduced, convinced me of the justice of my suspicion. My patient would not hear of operation of any kind, and even opposed a repetition of acupuncture, and yet he desired to be perfectly freed from his disease. I now tried the external application of tinct. of iodine for three weeks without success. The dis-

ease, and the operation proposed for its removal, almost set him mad ; fear of trusting himself to unskilful hands in his own country prevailed at last, and he determined on submitting to operation on the 1st of November. There was not more fluid evacuated through the puncture than two spoons' full, and then ʒi. of tinct. of iodine, in ʒiii. of water were injected. He complained of such violent pain that I was compelled after one minute to evacuate the fluid, but I purposely left some in the tunica vaginalis. After two hours the pain became so violent that he lost his consciousness and fainted, and did not recover for some hours ; the pain had then disappeared. In the evening he felt well, and there was no fever.

November 2nd. The tunica vaginalis is remarkably swollen.

5th. Tumour in same condition, and on moving it there is a crepitus felt as in emphysema.

7th. Patient very unhappy that the tumour has not decreased ; he is apprehensive that fluid may have collected anew, and thinks that he feels it. He has become more courageous, and desires to be operated on again. I evacuated through the puncture somewhat more than a spoonful of fluid of the colour of the former injection. The tumour now quickly diminished. On the 21st of November he was perfectly well.

CASE VIII.—J. Wachler, cabinet maker, æt. 28, had a hydrocele of the right tunica vaginalis, of the size of a fist, and of the form of a pear for eight months ; on the same side there was a varicocele.

On the 18th of January I injected ʒii. tinct. iod. aq. font ʒvi. and allowed the injection to remain more than five minutes ; there was not much pain, and on that account I rubbed the coverings of the testis, and lifted up the scrotum in order to bring the fluid into contact with every part of the tunica vaginalis, on which the pain increased, and then I removed the rest of the injection. The testis felt healthy and not enlarged.

19th. Extensive inflammation of the scrotum ; the pain yesterday, an hour after the operation, increased, and for four hours was very violent.

24th. Pain and irritation very trifling ; scrotum somewhat smaller, and absorption appeared to have commenced.

February 4th. There is still considerable tumefaction ; rubbed the scrotum with tincture of iodine.

14th. The scrotum of its usual size ; patient perfectly well. The testis feels harder and firmer than on the side which has not been operated on. In June, the side operated on was still perfectly healthy.

CASE IX.—Olwig's child, a boy of 7 years ; scrofulous ; had, according to his parents, a hydrocele from his earliest childhood, which at first was confined to one side, (which they could not tell,) but afterwards it regularly had extended itself on both sides. For the first time, in the beginning of 1837, was medical aid sought. The examination gave evidence of a tumour on each side, distended, elastic, and manifestly a watery collection in the tunica vaginalis testis, the outer coverings of which were unaltered, and which extending from below upwards to the abdominal rings on either side, had pushed the little testis upwards and backwards. Neither effort, nor coughing increased the tumour, nor did a change from the horizontal to the perpendicular position cause any perceptible change of volume. As the general health of the boy was good, Dr. Magnus, his attending surgeon, contented himself with puncturing the hydrocele, and giving vent to a quantity of bright and clear watery fluid in a full stream, after which the tumour collapsed. Yet, after a few days, the dropsy had collected to its former extent ; and it was now at different times punctured in ten or twelve different points ; external applications of various kinds were made use of, and medicines, given in regularly increasing doses, administered. Also fomentations with a strong solution of sal ammoniac, tinct. iod., acetate of squills, and camphorated spirit ; frictions with mercurial ointment, with the oxymur. hydrarg. in form of ointment, with iodine ointment, with gradually increased doses of iodine, and the addition of tinct. digitalis. balsamic fumigations : all without any lasting

advantage being gained, until at last, after twelve months, the patient assented to the operation which had been proposed by Dr. Magnus, and I, by his wish, on the 4th of April of this year undertook the injection with iodine. I selected the right side as the one most distended; when the puncture was made, the boy was so violent, that it was with the greatest difficulty that I succeeded in throwing in the injection; and as the boy continued to screech, and to complain of violent pain, caused by the immediate action of the remedy, and as I had no experience in such young subjects, I determined quickly to draw off the injected fluid. No pain of any consequence followed, at least the child became quiet shortly after we went away: there was very little reaction, but a remarkable intumescence of the scrotum came on, which went away by degrees, but with the disappearance of which, the dropsical collection came on again. When I saw the child on the 21st April again, the hydrocele was not so large as formerly; and inferiorly around the part where the puncture had been made, there appeared to be an adhesion of the testicle and tunica vaginalis. On the whole the malady was very little improved, and I repeated the operation on this account, in concurrence with the opinion of the attending physician, and with the assent of the parents. I allowed the injection to remain this time longer in the tunica vaginalis, rubbed it upon the testis, caused the injection frequently to return into the caoutchouc bottle, and then injected it afresh, so as to distend the sac, and finally emptied it entirely after five minutes. Great tumefaction, without any violent pain, succeeded; in fourteen days every trace of the tumour had disappeared, and the scrotum had collapsed to its original size; even on the opposite side the dropsy had disappeared. The boy was perfectly cured of the disease on both sides.

Of the remaining six cases, four were of from four to eight months' duration; two caused by injury, contusion and pressure on the testis; one from a rashly suppressed gonorrhœa in a patient who suffered also from stricture; one cause unknown.

Sizes varied from a hen's egg, to that of two fists. The cure was accomplished in from two to five days: in from fourteen days to a month, the testis and scrotum had recovered their natural size. I have not seen these patients since, and, therefore, cannot be perfectly sure of their having remained well. In the cases communicated, we have before us the favourable results of the injections of iodine, both in children and grown-up persons of every age, in large and small hydroceles, when the disease was acute, and when chronic; when occurring on one or both sides, both in cases where the operation was undertaken for the first time, and in those in which puncturation had often been performed; nay, even in one case, where the wine injection had in vain been employed by a skilful hand; lastly in pure, simple, hydrocele, as well as when complicated with hæmatocele. (Hydro hæmatocele.)

In none of the cases treated by me, was the reaction so violent as to require recourse to any internal or external remedies. The greater number of my patients were able to pursue their wonted employments without injury in three or four days, many even in shorter time than this, and some on the very day on which the operation had been performed. In three to four weeks, and at latest after six weeks, every trace of disease had disappeared from those who had been operated on. Many were perfectly cured in a fortnight.

These favourable results have determined me in all cases in private practice, where I have no reason to suspect a disease of the testis or its coverings, to have recourse to the iodine injection, and to reserve the previously practised operation of incision, or laying open the sac, for those cases where probably the hydrocele is complicated with a degeneration, or some other disease of the testis, or of the tunica vaginalis.

I cannot conceal from myself, that the list of cases which I have given, differing so essentially from the results obtained by most distinguished surgeons, might awaken distrust and doubt on the accuracy of my observations, or even of the truth of their

author. Supposing such to be the case, I would say, that I have chosen my cases most particularly; and wherever I had reason to suspect any complication, or where there was any reason to render this operation unfit, that I never undertook it. In the fifth case alone I undertook the operation against my better judgment, and as there was no result, the prognosis in all my cases may be considered favourable. Five cases also offered themselves, in which I would not undertake the operation; two of them were in children who had congenital hydrocele, in both could the water be passed up into the abdomen; and in one of them it was forbidden on account of hydrocephalus, a disease to which the child some time after fell a victim; one patient advanced in life suffered from hydrothorax; another induration of the testis. In the last the testis felt irregular and uneven; this person had, as was demonstrated by the radical operation, an enlargement of the testicle, with hydatids in the thickened tunica vaginalis, and a hydrocele of the spermatic cord, and could not have been cured of his disease by injection.

In the cases on which I operated, one or other of the following gentlemen were present, had the goodness to assist me, and were convinced of the favourable results of the operation, Drs. Gunther, (now professor in Kiel), Herzfeld, Magnus, Moench, Morath, and the anatomist Lessing.

It is easy to be explained, how that different observers have arrived at very contradictory results in the application of this remedy, some considering it useless on account of the trifling reaction caused by it; whilst others, on the contrary, thought it acted too violently, so as to cause suppuration and gangrene of the scrotum. The tincture of iodine is very differently prepared in different pharmacopœias; in some there is more, and in others less of the iodine dissolved, (the *Hamburgh* as well as the *Prussian* says, forty-eight grains of iodine to one ounce of rectified spirit; the *Parisian* only forty grains.) By the less degree of diffusibility of the iodine in water, the iodine con-

tained in the tincture is very soon precipitated when mixed with water, and it is the more the longer it may remain, so that at last there is nothing but a mixture of water and alcohol. A preparation which unites in such different proportions, cannot be expected to produce similar effects; I find it then most expedient to mix the ingredients at the time of using them, and to make the degree of pain suffered by the patient, the criterion for leaving the injection a longer or shorter time in the tunica vaginalis. The mixture may also be applied warm, for then the iodine will remain longer united with the spirit. By the employment of the caoutchouc flask in place of the simple syringe, there is a double advantage to be derived, the permitting of the fluid to pass many times back and forwards, without any danger of extravasation into the cellular tissue, and by shutting out the light to prevent the separation of the iodine.

S. L. L. BIGGER.

ART. XI.—*Simple and Complete Dislocation of the Astragalus from the Os Calcis and Navicular Bone, upon the Dorsum of the latter, without disturbance of the Relations between the Tibia, Fibula, and Astragalus, and without Fracture of any of these Bones; the Case of Richard Carmichael, Corresponding Member of the Académie Royale de Médecine, &c.* By JOHN MACDONNELL, M.D., one of the Surgeons to the Richmond Hospital and House of Industry, &c.

THE following case possesses a high degree of interest on several accounts: on account of the extreme rareness of the accident; on account of its having happened to a medical man, capable of giving strictly correct information respecting every particular relating to the accident and its consequences; and on account of the individual to whom it occurred. Independently of its claims

because of its rareness, and the completeness of its history, this case will be received by the profession with a deep, and, I think I may add, an affectionate interest, wherever British surgery, and the genius by which Mr. Carmichael has so largely contributed to improve and advance surgical science, are held in honour. For myself, bound to him, my master and my friend, as I am, by the strongest ties of obligation and of respectful attachment, it is a source of pride and gratification to me to record the fortunate issue of this severe injury; and that I had any share in contributing to this issue, I shall, while I live, regard as one of the happiest occurrences of my life.

On the evening of the 6th of last August, Mr. Carmichael was riding in the immediate neighbourhood of Dublin at a brisk trot, when his horse suddenly fell. The abrupt stop was calculated to precipitate him forwards, which he prevented by throwing himself back in the saddle, and strongly extending the foot, leg, and thigh, to meet the ground. The shock of his descent was accordingly received upon the anterior extremity of the metatarsal bones, especially that of the great toe of the right foot, which alone came to the ground. The inner edge of the foot was slightly inclined downwards and outwards; for on examining the boot, the part of the upper leather on the inside ball of the great toe was found covered with mud. In a few seconds the animal sprung to her feet, carrying Mr. Carmichael, who did not lose his seat, along with her. Heavy tensive pain, which almost caused fainting, and deformity, obvious through the boot, convinced Mr. C. that he had suffered a dislocation of the foot. A humane gentleman, a stranger to Mr. C., who was passing in a car, gave it up to him, and lest he should require help, followed him on foot, a distance of nearly three miles, to his house. On his way thither, he called at the shop of Mr. Beatty, apothecary, Sackville-street, and requested him to summon Dr. Hutton and me to his assistance. We were at his house immediately; indeed I was there before himself, and on getting after his arrival some idea of the injury, I had his bed properly

prepared with mattress, &c. without delay. Having carried him to bed, and having had him undressed, we were now enabled to inquire and examine minutely into the nature of the injury. The following was the deformity we observed : the toes were turned outwards, the inner edge of the foot forming an angle of about 30° with its natural direction ; the sole was slightly turned outwards, and the outer edge slightly elevated. The concavity of the tendo achillis posteriorly, was manifestly increased, and the heel lengthened. On grasping the soft parts between the tendo Achillis and tibia, we found the distance between these parts much greater than in the other foot. The absence of the hard projection which would have been formed by the upper articulating surface of the astragalus, had it passed backwards with the other tarsal bones, was evident. The malleoli were perfectly defined. Below and before the inner there was a hard prominence, over which the skin was tense, formed by the inner surface of the astragalus, brought into relief by the dislocation and the slight eversion of the sole of the foot. Much the most striking part of the deformity consisted in a prominence on the dorsum of the foot. Immediately in front of the tibia it presented a flat surface, broad enough to receive the finger, and from which there was an abrupt descent upon the anterior part of the tarsus. Over this projection, caused by the head of the astragalus thrown on the upper surface of the scaphoid and cuneiform bones, the integuments were so tense, that it was evident, a very small additional force would have driven it through the skin : lastly, on taking the distance from the point of the internal malleolus to the extremity of the great toe, with a tape measure, I found it to be nearly exactly an inch less than the distance between the same points in the left foot. We could detect no fracture. The foot could be flexed and extended ; but, as any movement occasioned great pain, we did not ascertain to what extent flexion and extension could be carried.

There was no obscurity about the deviations from the natural form just enumerated. The accident had happened about

half an hour before. There had not therefore been time for inflammatory tumefaction to arise, and no effusion whatever of blood or other fluid had taken place. We concluded the dislocation to be that designated in the title of this paper.

Before describing the mode of reduction, I shall endeavour to establish the rationale of the accident. In the production of it, there were, I conceive, three principal forces in operation. 1st. The dash of the anterior part of the foot upon the ground, (the foot and whole limb being strongly extended), which tended to drive the foot upwards and backwards. 2nd. The powerful effort of the posterior muscles of the leg, which dragged the foot in nearly the same direction. 3rd. The shock of the weight of the body descending with violence, and aided by part of the effort of the posterior muscles of the leg. This force, by the strong extension of the foot, gained a purchase on the back part of the upper articular surface of the astragalus, and was thereby enabled to unseat it from the calcis, and jet it on the dorsum of the scaphoid. I think also, that a consideration of the action of the extensor longus pollicis upon the astragalus, where its tendon passes through the groove in the posterior part of that bone, will convince us, that it was especially effective in throwing the astragalus upward and forward.

There are, in the action of the posterior muscles of the leg when they raise the body on the toes, some peculiarities, which came into play in this accident, and are worth adverting to. Though what I shall have to say would apply, *mutatis mutandis*, to the deep muscles, I wish to confine attention to the gastrocnemius, plantaris longus, and soleus muscles. 1st. These muscles, in raising the body, and in the accident they did the same, act on the foot as a lever of the second kind, the fulcrum being placed at the anterior extremity of the metatarsus, the weight acting at the ankle joint, and the power at the posterior extremity of the lever. This, as is well known, is a kind of lever of which few examples are met with in animal mechanics. It contributed essentially in determining the nature of the acci-

dent, by enabling the gastrocnemius, &c. to maintain the heel elevated, by the mechanical advantage it afforded them over the weight. I look upon it as certain that the heel did not come to the ground; if it had, (the shock, as will be recollected, having been received by the foot with its outer edge somewhat elevated, and the inner depressed,) the violence would inevitably have resulted in the common compound dislocation of the tibia inwards, with fracture of the fibula near its malleolus, and perhaps the tearing up of a shell of bone from the tibia by the inferior interosseous ligament. 2ndly. In muscular action, the part of the contractile effort expended upon the fixed attachment of the muscle is usually simply lost, neither adding to nor diminishing that which acts at the moveable attachment. This is not the case in the elevation of the heel; and I think it is demonstrable, that in the dislocation, the whole enormous effort of Mr. C.'s powerful gastrocnemius, &c. was effective in the production of it. In all cases of muscular action, half the effort acts at the origin, and half at the insertion of the muscle. In general the former is either lost by the absolute fixedness of the origin, or neutralized by other muscles. But in the case under consideration, while half the muscular effort raises the heel, the other half draws down the tibia, &c. and is therefore not to be looked upon as lost with regard to the elevating half, but as what an algebraist would call a minus quantity, to be subtracted from it. Thus supposing the gastrocnemic muscles to exert a force equal to 120, the heel will be raised by a force as 60, and the tibia, &c. drawn down by a force as 60. But if so, it may be asked, how comes it that the one does not neutralize the other. Because the former acts on a longer lever than the latter, and therefore has a mechanical advantage over it. By a simple calculation we may make a near approximation to the truth, respecting the part of the elevating actually neutralized by the depressing force. Thus, supposing the distance from the anterior extremity of the metatarsus to the middle of the astragalus (where the depress-

ing force acts) to be to that from the same part of the metatarsus to the posterior extremity of the calcis, as 2 : 3, then the depressing force equal to 60, acting at the astragalus, is equivalent to, or neutralizes a part of the elevating force equal to 40 ; since these numbers are inversely as the levers by which the forces act, and these consequently balance one another. The remaining part of the elevating force, equal to 20, *i. e.* one-sixth of the whole muscular effort, is that by which the weight of the body in ordinary cases is raised, and by which, in the accident, the violent descent of the body was more than counterbalanced. I have entered into this analysis because it shows clearly how enormous the whole force exerted by these muscles must have been ; and as one-half of it was employed in drawing up the calcis, and the other, in conjunction with the shock of the body, in gouging (if I may use the expression) the astragalus from its bed on the calcis the inferior calcaneo-scaphoid ligament and scaphoid bone, it is plain that it must have mainly contributed to effect the dislocation.

In this injury the synovial capsules between the calcis, astragalus, and scaphoid were torn, and the following ligaments must have been completely ruptured ; the middle division of the external lateral ligament, running from the point of the external malleolus, to the upper and middle part of the external surface of the calcis ; the part of the internal lateral ligament which is attached to the inner part of the calcis, and sheath of the long common flexor of the toes ; the posterior and external ligaments, the one connecting the posterior edge of the astragalus, the other, the part immediately below the external malleolus, to the neighbouring parts of the calcis ; and lastly, the powerful interosseous ligament, occupying the interval between the superior articulating surfaces of the calcis, and running to the corresponding deep groove between the inferior articulating surfaces of the astragalus ; the astragalo-scaphoid ligament connecting the neck of the astragalus to the upper surface of the scaphoid bone was torn, but I believe some bundles of its fibres,

especially those running to the cuneiform bones, were not ruptured.

Having satisfied ourselves respecting the nature of the dislocation, and that the proper manœuvre (after flexing the leg strongly on the thigh, to disarm the posterior muscles of the leg, and making extension, and counter-extension, at the foot and knee) was, to press the heel forward, and the astragalus and tibia backwards, while the toes were drawn inwards, and the outer edge of the foot somewhat depressed, Dr. Hutton and I tried our full force upon the foot successively, putting Mr. C. to great torture, but without the slightest good effect. We therefore resolved, with his concurrence, to have immediate recourse to the pulley apparatus, which Dr. H. procured from Mr. "C.'s Armamentarium." Having buckled a strap, on the part of which corresponding to the sole was a pad with an iron ring firmly attached to it, round the foot, immediately in front of the astragalus, and having placed on the heel a piece of girth-web, the ends of which were crossed on the dorsum of the foot, and then made fast to the ring, he attached the pulleys to this. The further end of the pulleys was well secured; and a padded strap, taken round the lower part of the thigh, was fastened to a bed-post. The leg, the outer side of which was downward, and which sloped gently upwards from the knee to the foot, was strongly flexed on the thigh. Mr. Beatty took charge of the pulleys, and made gradual and steady extension for about ten minutes, occasionally retaining the force at a given point for a few seconds. Dr. H. placed one hand on the heel, and grasped the fore-part of the foot with the other, prepared to, perform at the proper moment, the manœuvre already described; while I, with one hand grasping, for the purpose of drawing it backwards, the lower extremity of the tibia, and with the other on the internal malleolus, for the purpose of depressing it, held myself in readiness to meet and favour his effort. Up to the moment of the reduction, Mr. C., with admirable fortitude, encouraged us to proceed, exhorting us "to do our duty"—"not to mind him." Mr.

Beatty, who, though not a large, is a strong man, had put his whole strength to the pulleys, (which consisted of a system of six pulleys in two blocks ; three of the pulleys were fixed, and three moveable, and consequently multiplied the force applied to them six-fold,) and owing to the pain his hand suffered from the cord, had drawn it over his shoulder, and thrown the weight of his body, in addition to his strength, upon it. The pain now became unendurable ; and while Dr. H. and I were intently engaged in the guidance of the bones, Mr. C. made a violent effort, which was sensibly felt by Mr. Beatty, across the pulleys. At this instant the pulleys were relaxed, and the reduction was effected without noise. All deformity, and the tensive pain experienced before the application of the pulleys, were gone, and the agonizing pain of the extension replaced by a feeling of uneasiness merely, about the ankle. His first remark was, that in any similar case occurring in his practice he would use the pulleys alone ; declaring, that we had put him to greater pain by the jerking unsteadiness of our first efforts, than he had suffered from the pulleys.

No case of similar violence ever did better. He experienced more annoyance at the ball of the great toe, which became swollen and ecchymosed, than at the dislocated joint. Twenty-five leeches were immediately applied, forty-eight the next day, and twenty-four the next. The ankle and foot were wrapped up in a large soft poultice of linseed meal, and stupes were occasionally used. He took no anodyne, being afraid that opium would, as it had often done before, disorder his stomach. On the second night he took a blue pill, and next morning a purgative draught, with grs. xv. of electuary of scammony. For some days his diet was of course strictly antiphlogistic. I may say he had not a bad symptom. On the fourth day, owing to the discontinuance of the poultice, the parts about the ankle became hot from cool, their veins full from nearly empty, and the skin tense and satiny from being shrivelled ; but these unpleasant symptoms soon disappeared on having again recourse

to the poultice. He expressed a decided preference for warm stupes, and warm and moist poultices, which kept the parts as in a vapour bath, and afforded him great comfort. He gave a trial to cold, which, however, he soon left off, finding them much less agreeable than warm applications. On the twenty-sixth day after the accident, he could lay a good part of his weight on the right foot, with but little pain; and on the thirty-third, I saw him stand upon that foot without any. To-day (the thirty-ninth since the accident) the tumefaction, from effusion into the synovial capsules of the ankle-joint and extensor tendons of the toes, &c., which was at no time very considerable, is much diminished; but the skin has not recovered its colour, nor the ankle its natural fineness.

A remarkable circumstance of the accident was, that Mr. C. was sensible of no pain at the moment of the dislocation. The rapidity of the injury is probably the principle on which the explanation of this depends. Injuries which we should expect to produce intense pain, if instantaneous, often produce little or none; as in the case, related by Dupuytren, of a man, who in passing through a foundry, placed his foot in a channel in which the molten metal flowed upon it at a white heat. The foot and ankle, bone and all, were charred, without his being conscious of injury. Probably no sense can appreciate an injury which acts but for an instant. With respect to vision, the curious phenomenon presented in the child's play, in which a rod, the end of which is red hot, is made to produce a riband, a circle, or a figure of eight of fire, by rapidly moving the ignited point, and several other optical illusions, are explained on the hypothesis, that an impression on the retina acts on the mind for about the ninth part of a second; and I think it follows, with regard to the perception of colour, for example, that a second colour, presented for an instant to the eye already occupied with another, cannot be perceived; because the latter will occupy the perceptive faculty for the ninth part of a second, before the end of which time the second colour is removed.

With respect to the rareness of the dislocation, none of my

surgical friends here have met with another case; and, after a diligent search, I cannot say positively that I have found a single case of the same accident. Sir A. Cooper, Boyer, Chelius, and Gibson, have not recorded it, which I look upon as good proof that it is not a recognized dislocation, admitted to occur by the surgeons of Great Britain, France, Germany, or America. Mr. Adams, who is known to have devoted much attention to the subject of dislocations, has met with no example of it in his own practice, but has referred me to the following case, related by Mr. Hey, as differing merely in being compound. “A case of compound luxation occurred in 1758, when I was a pupil of St. George’s Hospital, in London. The patient was a corpulent woman, who in alighting from a horse on which she had been riding single, happened to catch hold of the stirrup with the heel of one shoe. In consequence of this she came down to the ground, upon the other foot, with so much violence, that the inferior extremities of the tibia and fibula, together with the astragalus, were forced through the capsular ligament. Mr. Bromfield, whose patient she was, finding reduction to be impracticable immediately amputated the leg; but the woman did not recover.” Perhaps this was a luxation similar to Mr. C.’s. I incline to think, however, that it was not. If it were, all ligamentary connexion between the dislocated bones being completely destroyed, the impossibility of reduction is difficult to conceive. But this impossibility is quite consistent with the opinion to which I incline, that it was the comparatively common luxation of the astragalus from all the bones it articulates with. It will be observed, that Mr. Hey does not say he saw it himself, and the case bears internal evidence of not having been recorded with the usual care of that excellent surgeon. It is obvious, that there is no capsular ligament through which the “inferior extremities of the tibia and fibula, together with the astragalus,” could be forced. “J’ai eu occasion,” says Boyer,* “de voir une luxation incomplète de la tête de l’astragale

* Traite des Maladies Chirurgicales, tom iv. p. 406.

en haut et en dedans, sur un homme qui avait fait une chute de cheval: le gonflement inflammatoire qui survint était si considerable qu'il m'empêcha de reconnaître ce déplacement dans les premiers jours; et lorsque je pus en juger par la tumeur légère que formait la tête de l'astragale, il fut impossible de remettre cette éminence dans sa place naturelle. Les mouvemens du pied furent gênés pendant longtemps, parceque l'articulation tibio-tarsienne avait souffert, une entorse considerable; mais ils se rétablirent par la suite, et il ne resta qu'une légère difformité." Dr. Tarral* quotes, from a manuscript by M. Roux, three cases of luxation, without fracture, of the head of the astragalus on the cuboid bone. These are the dislocations most nearly approaching to Mr. C.'s, to which my search for such cases enables me to refer the reader.

ART. XII.—*On Animal Magnetism.* By ANTI-QUACK.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

SIR—This age must be beyond all others the age of quackery and imposture. Never before have such experiments been made upon the temper and patience and common sense of the sober and really rational part of our species. The follies and freaks of the phrenologists might have sufficed to occupy the credulous, while their doctrines remained proof against reason and ridicule, but mesmerism and magnetism have come upon us accumulated, and contemporary, and claim a divided empire with the unproved and unproveable, and if I may use the phrase, undisproveable follies of the phrenologist. Indeed, Sir, the fact that such formidable assaults have been, and continue to be made upon the understandings of the public by men of a character which *would* be respectable but for their repeatedly vouching, what till now had been unhesitatingly de-

* Cyclopædia of Practical Surgery—Art. Ankle.

clared incredible because impossible, renders it incumbent on men of science, who profess to be conversant with logic, metaphysics, and the physiology of man mental and physical, to favour the public with some intelligible criterion by which we may be able to discern the limits which divide the *possible* from the *impossible* and the *credible* from the *incredible* in matters connected with man's material and moral nature! Perhaps, Sir, as *you* are, I presume, a member of that undoubtedly learned and most multitudinous body, the British Scientific Association, you might obtain from *them* a solution of this important problem, and thus prevent our incurring the disgrace of assenting to those *apparently* absurd, incredible, and impossible allegations which have lately been inculcated upon us by your brethren of the healing art; inculcated, until at last we have been *almost* persuaded of their credibility and truth from the frequency and confidence with which they are asserted.

Sir, in a former number of your valuable work, we laughed at some of the many phrenological whims of Dr. Elliotson, in one of his then recent publications. The learned gentleman has become no less remarkable, lately, by his experiments in magnetism and mesmerism, than by his phrenological labours. I believe, however, I should not have now troubled you with any observations on his errors and follies, his discovery of the power of *nickel* to mesmerise and somnambulize, or the comical exposure of it by the sagacious ingenuity of Mr. Wakely, which so demonstratively proved, by a single experiment, the credulity of the Doctor and the frauds of his instruments! I should not, I say, have now again solicited a page in your Journal on that head, but that, as if even yet something had been wanted to make the exposure of these follies complete, we have recently been called on to witness the inconceivable absurdity of some mesmeric attempts at imposition which appear to have attracted the attention of several of the faculty in Paris; and, wonderful! even that of the *collected* wisdom of that learned body, the Academie

Royale de Medicine, in that city ! I know not, Sir, whether the last Monthly Report of the Royal Academy has reached you, but I have accidentally met the second part of that Report, or *Bulletin* as they call it, for the last month, August, in which is given a detail of proceedings taken by a committee appointed by the Academie to consider and report on a certain mesmeric or animal magnetic somnambulist case ; one, exceeding perhaps in its incredible absurdity, even the most absurd of the incredible cases with which the medical magnetic theorist of London had yet made trial of the extent of public credulity !

The Report of this “ *Commission sur le magnetisme* ” may be found in the 62nd page of that Number. It does not contain any determinate opinion of the Academy, or of the committee on the *truth* or *fallacy* of the pretended power. It gives only a detailed account of the steps taken by the committee to insure a true test of this extraordinary case, which was shortly this :

A very young female had been brought to Paris by her father, who asserted, with the utmost confidence, and courted inquiry, that the girl, a somnambulist, could, when *mesmerised*, and solely through the animal magnetic influence, READ the contents of any given writing, if placed before her while under the magnetic power, and though her eyes were so bandaged as to preclude the possibility of VISION !

It appears by the Report, that one of the members of the academy, a M. Burdin, in order to afford a full opportunity to ascertain the truth or falsehood of this alleged and extraordinary power of magnetism, proposed to give a prize of 3000 francs to any individual who should be able to *read* without the aid of *light*, of the *eyes*, or of *touch*. The Royal Academy sanctioned the offer of this prize, and undertook to superintend, direct, and decide on such experiment as should be made in consequence of it, and to award the prize to the person who should be found to deserve it : and they appointed a special committee of their body to superintend such experiment. Though this prize was offered to any candidate

who should appear, it was known that the candidate would be the somnambulist daughter of Doctor Pigeaire, a girl of the age of twelve years, whose mesmeric power to read without vision he had confidently and industriously published. The committee therefore, apprized M. Pigeaire of the resolution of the Academy : the Academy, with M. Burdin's assent, having previously declared that though *touch* was excluded by the original programme, yet they would consent that the *touch* might be used, provided it was not used in aid of or as supplementary to vision. It was also agreed that the committee were to be invested with the power of prescribing such cover or bandages for the eyes as would render vision by the eye impossible ; the object to be read however, being left in full light.

Nothing could be more fair, or more likely to put this pretended power to the test of truth. M. Pigeaire was formally apprized of those terms ; he admitted the receipt of the official letter in April, 1838 ; and on the 26th of June, the committee received a letter from him, stating that he had arrived in Paris to exhibit to the Academie the " very extraordinary magnetic experiment" which had been announced by him, &c. and which he again stated to the Academie in this letter, in a passage thus translated :

" This experiment consists in the following fact : A child, aged 12 years, being subjected to magnetic action, or *power*, will read, the eyes being covered by an opaque bandage, and there being placed a plate of transparent glass on the page of the book in which she reads."

As the *fraud* of this transaction must depend upon what the party undertaking to perform, did actually perform, I must trouble you by adding another part of this letter stating what he undertook. It is as follows :

" I shall lay before you, Gentlemen, (said the Doctor,) the bandage (*appareil*) which is applied to the eyes, in order that you may be assured it is impervious to a single ray of light."

And then he describes a bandage, formed of three folds of black velvet and pledgets of cotton, to apply directly to the eyes ; the whole covered with a double band of fine linen, all to be so applied as that the eyes shall be in the most profound darkness, “ and if any doubt should remain on your mind, as to the thorough opacity of this covering, you may make another of the same form but constructed of the same materials, as we know that velvet is not adverse to the magnetic action.”

There are other stipulations stated in his letter, such as that the mother is to magnetize the girl ; that the mother should be the person to apply the bandage, but that the committee might touch it with the fingers, &c. ; that they might be convinced that the eyes were covered with the bandage, and reading by aid of the eyes impossible, &c.

The committee answered this proposition of Doctor P. recalling his attention to the former communication they had made to him, and apprizing him that M. Burdin, who gave the prize, had also fixed the *conditions*, that they could not then be varied as he desired ; and particularly, that the form of the mask by which the eyes were to be covered in the experiment, was at the discretion of the committee.

M. Pigeaire, in his answer, agreed to meet the committee and proceed with the experiment on the day fixed. He and his daughter *did* meet them, and from what then took place, you, readers, may form a tolerably just estimate of the *fair dealing* on which mesmeric experiments rest ! I shall make no comment on the facts disclosed by the following statement which is given by the committee in their Report to the Academy.

“ The president of the committee having been prepared with a bandage, such as had been thought by them fit to exclude vision, he asked Doctor P. whether the young lady could read when the bandage which he, the President, produced (and which the Committee then laid before the Academy) should be applied to her eyes.

“ The Doctor, after having examined the bandage, said the the bandage should be applied closely (*immediatement*) on the

eyes ; that if there existed the least void space, or if the light should penetrate between the eyes and the bandages, the experiment could not succeed. And," he added, "that the case would be the same *if a sheet of paper were interposed between the bandages and the object to be read !*"

The committee then proposed to the Doctor, that he should take the bandages prepared by the committee, and adapt to its interior whatever he might think proper, to fill any void interval, and that the mask should then be applied closely to the eyes.

The Doctor, (who then, no doubt, plainly perceived that this bandage of the committee would effectually prevent the young lady from *peeping !*) said "that they had, in vain, tried several species of bandages, and that *all* of them had produced nervous fits, more or less intense, on the young somnambulist, that the bandage or mask, which *he* now presented was the *only* one proper for the experiment, and that *no other* could be used !"

Here I submit to you the *fraud* of the whole transaction became apparent, and that further observation was needless !

The President however then proposed, (to meet, I presume, the nervous irritation of the young lady from *bandages*,) that an opaque moveable veil should be used which should cover the eyes and the greater part of the face. But the Doctor opposed this also, and said, that the form of *his* mask could admit of no modification ! FOR, that the cheeks or jaw (*les joues*) should remain uncovered, he being inclined to think, with many other persons, that his daughter was enabled to read by means of the *nerves* which are distributed over *that* region of the face !

The committee then, apparently anxious to give every possible advantage to the mesmerist, proposed, by one of its members, that in order not to deprive him of the benefit of the cheeks, there might be too conical tubes prepared, with one end of each *larger* than the other, so that the larger orifice of each might be opposite the object and admit the light, the

influence of which might, through the other end of the orifice, in contact with the cheek, make its impression : but this also the Doctor refused.

This might well have terminated the labours of the committee, for no rational doubt could remain that the pretence to the alleged power was a pure imposition. However, the committee put some further questions, which tend to throw still more light on the tricksters.

The president asked, whether the young lady could read *immediately* after the doctor's own mask was applied to her? The answer was that she read more or less promptly at different times, and that the time varied between a quarter of an hour and an hour and an half!

To an observation which had been made, that the girl, after the application of a bandage, repeatedly, and with more or less violence, moved the muscles of her face : the Doctor said, she did use that movement sometimes and sometimes not.

The committee then inquired as to the particular position in which the book to be read was held ; and whether she could read if the *book* were held up so as to be on a level with the eyes?

To this he answered, that the book must be placed either on the girl's knees, or on a table before her ; and that she must have liberty to place it where she pleased ; but certainly the book could not be placed before her at the same height as the eyes.

And finally the doctor declared, that if *he* consented to any of the committee, during the reading, applying their fore-fingers to the inferior border of the bandage, his daughter not only would not read, but would fall into a state of convulsion more or less severe ; that his daughter read best before a *small* number than a greater, and that for that reason he had requested the committee to be divided into *two* sections.

Towards the end of the sitting the Doctor observed that at some times the girl read in the interior of a closed box, (*dans*

l'interieur d'une boîte fermée.) The committee unanimously declared that such an experiment would be conclusive if performed before them. The Doctor then said he was *not sure* she could read in that manner on *that* day!

The committee conclude their report by saying, "Such, gentlemen, are the explanations given by Dr. P. You must judge whether, if we have not proceeded any further in experiment on this subject, it is not because after having strictly conformed to the precise terms on which the prize was offered by M. Burdin, we have not found, either in the form of the bandages offered by Dr. P. or in the manner in which the book must have been held by the somnambulist, any positive certainty that the light might not reach her eyes."

Here then is mesmeric imposture again detected and exposed in a manner, I hope, which will go far to set the public mind at rest on this subject. The lovers of the marvellous have indeed lost the gratification of one instance at least of wonder; but, with all the respect which I feel for the committee who have, with so much good sense, stopped the progress of this fraud, I cannot help thinking that the Academy itself has furnished a subject for *wonder* almost equal to that which they have removed; for, is it not surprizing, that that very learned body, or the committee whom they appointed, could have hesitated for a single moment in pronouncing, on the mere statement, the whole affair an impudent attempt at imposition on them and the public?

How, indeed, can we account for it, that a respectable scientific body, an assembly of French Medical Professors, incorporated by Royal Charter as the *elite* of a corps of scholars whose professed object of study is the physiology of man, should have condescended to notice this incredible pretension! one, which I hope, to *your* readers, as to me, appears on the very statement of its terms, to be audacious and contemptible quackery! and that on its mere announcement its incredibility was apparent. For what was the naked proposition? It was this: that of

a written paper, never seen by the young “*artiste*”, (as I presume they will denominate her in France,) of the contents of which, at the moment she was utterly ignorant, nay, even of the language in which it was written or the subject of which it treated, and of which the whole was to be withheld not only from her sight but her *touch* also as auxiliary to sight—she should fully, truly, and really READ the contents as if submitted to her perusal with open vision! This, I do conceive, must, on the mere statement, strike every thinking person as INCREDIBLE, *because impossible!* and for the following very obvious reason, namely, that to *read* without *vision* involves a contradiction—for what is *reading*? READING, properly so called, must be by VISION, or by tracing the written characters through or by some known sense, as *feeling, touch, &c.* But, as to the sense of TOUCH, or the possibility of the performer achieving her task by any alleged exquisiteness of that sense, no question could or did arise; for, by the stipulation it was not by touch the marvel was to be wrought, nor was it suggested or understood on any hand, that touch or feeling should be a substitute for, or aid to vision; indeed it was rendered impossible by the plate of glass placed on the object to be read. Thus, the *only* question was, whether the mesmerised patient *could read the written paper without any other substitute for vision than her mesmeric and somnambulist faculty?*

When the somnambulist, therefore, undertook to *read* a writing thus produced, but concealed from her vision, she must be taken to mean that she could give the contents of the writing *as if read with the eye*—though vision *by the eye*, or any known substitute for it, was precluded: that is, she was to READ, which requires, in the common meaning of the word, VISION, without the aid of the essentially REQUISITE ORGAN! was to perform an *organic function*, without the ORGAN! and give the contents of a *written document* to which the eye is *necessary*, without that *necessary requisite, or any known substitute for it!* This perhaps will

appear yet more clear, by recollecting that the very *condition* of the experiment was, that in order to the reading—in order to perform the things to be done—there must be a *writing* submitted and *placed before her* when blindfolded ; for she does not pretend to *state or communicate* any thing but that *which is written* and before her ; no extrinsic facts, or *future* vague prediction, or indefinite statement of a *past* event. She pretends to *read* only, but *suo more*, and without *vision*. The whole was to be a *strict*, and properly so called, *reading* of a written document concealed from the eye—and by the aid, not of an organ, but a *visual* function performed without an *eye* !

I repeat, therefore, that the very terms of the proposition to read a written paper without sight, imply a direct and necessary contradiction ; and therefore *per se* are incredible and impossible.

The magician, the necromancer, the dealer in miracles express and avowed, might naturally enough pretend to perform such a feat ; but, it must be presumed, that of such gentry the “ *Académie Royale de Médecine*” of France would not take notice—at least would not issue a commission to members of their body to investigate and report on the *truth of such pretensions* ! And yet, is there any real difference between the tricks of the magician, the fortune-teller, or the miracle monger, and this obvious and palpable, this self-detecting imposition of the magnetised somnambulist !

Again, let us see how this view of the case is corroborated, by considering what *is* or *can* be, this *Animal Magnetic Power*, if there be such an entity in nature. It is not pretended to be a spiritual, intelligent, invisible, incorporeal power, different from, above, and beyond the ordinary laws of the nature of man and matter, and therefore a subject of *religious* belief, not of scientific or philosophical investigation. It is treated of and experimented on as a *material quiddity* of some kind, and deriving its existence and power from *material* sources—indeed from so *low* a source as a species of *manipulation*, or one which would appear to be very nearly allied to a *metallic* origin,

if, as Dr. Elliotson teaches, the metal *nickel* is the great origin or instrument by which the power is executed or produced, and from which that “*beautiful series of phenomena!*” proceeded, which dazzled for a time the public eye, and bewildered the popular mind, until Mr. Wakley’s successful *manipulation* of the *lead* and the *nickel* exposed the ridiculous deception, and *for so much*, deprived Dr. Elliotson and the medical savans, his disciples, of this *nickelian* source of honour and enjoyment!

Sir, can it be necessary for me to say, that I do not deny but that, as a *material* agent, magnetism, perhaps *animal* magnetism, may act on matter subjected to its power, and *that* in a way, and to a degree, for which we may not be able to account? It *may* produce many singular, nay, surprizing effects. Let it be conceded that it *may* produce contortion and convulsion in mesmeric patients on whom it may be made to act; and even, if you please, act upon living subjects as galvanism upon a dead one. But its power must be confined to *that region*—to *material* action upon *matter*. It cannot, therefore, supersede the fixed laws which regulate animated nature; it cannot displace or supersede the principles by which organic power acts on, or in, intelligent agents; it cannot substitute a *new* physiology for man, and displace that which the accumulated experience and diligence of all past generations have left to us; it cannot produce *intellectual* results by modes inconsistent with those principles that have been ascertained by the wisdom and proved by the experience of innumerable ages; it cannot, as a material agent, create or open new entrances for knowledge *in lieu of the senses* or the *organs of sight and hearing*; it cannot, therefore, enable us to *read* what, *as to us*, was never written, or, *as in the particular case* under consideration, had never been within the reach or knowledge of the pretended “READER” without “VISION!” For, again I say, that *reading* is a joint operation of the *sense of sight*, and of the *intellectual power* of the reader. If there be a mode of getting a knowledge of the precise contents of a written paper, without

the intervention of the eye, that mode cannot be reading, and in such a case the writing itself is perfectly useless, totally superfluous ; for it can be of use only that it may be SEEN ; it can apply to no other sense ; it is calculated for no other ; and, therefore, to read without vision must necessarily be tantamount to reading what was never written, and of course to reading what, if once written may have been lost, destroyed, or so concealed or removed from the operation of the sense as to be virtually a nullity.

Sir, perhaps after all these reasonings of a very plain man, though they appear to *me* conclusive against this asserted power, still as the precise limits of possibility have not yet been fixed by the scientific body to which I have already referred you, we may hereafter have to boast this new accession to the power of man ! May we not then innocently indulge, for a while, anticipating the incalculable benefits we shall derive from this mesmeric faculty ! For instance—as the *presence* of writing cannot be necessary to such a *reader* as this somnambulist infant, may not her power, should it exist, give us reasonable ground to hope that she may yet restore to us, by this her *reading faculty*—what the learned world so much desire—the whole of the lost writings of Livy, of Cicero, and of the rest of the now mutilated classics ! may she not without further expense or trouble, procure us perfect copies of all the Herculean and Pompeian manuscripts—supply to us the desiderata of history ancient and modern, by disclosing the historic documents which the barbarism of early conquerors has destroyed, or the frauds of rulers, or of partial historians, suppressed from posterity ? or which (a more limited exercise of her wonderful power) may now be buried in the dust of neglected, forgotten libraries ! All this we may surely hope from the stupendous faculty of this mesmeric adventurer ! For neither the *sight*, nor the actual *presence*, nor, therefore, the *present existence* of any writing which was once in *esse*, can be beyond her all-searching penetration ! or, do any of the *Académie Royale*, or of those who (unlike *them*) are willing to countenance this impudent impos-

ture, deny that the power of reading without *sight* a concealed writing, does actually, if it exist at all, go the length I have above stated? If they do, I pray them to state intelligibly how far, if at all, beyond the reading of the individual writing concealed from her, does this pretended power go? If she can *read* what she has not *seen* in the paper before her, why is the *presence* of that paper *necessary* to her reading? Could she not, and *why* could she not, read it, if removed to an adjoining room? and if to *that*, why not if removed to the *Bodleian Library* or the *Vatican*? And yet again, if she can read it while *there* existing, why may she not if the paper was yesterday destroyed? and if she can read *to-day* what *yesterday* ceased to exist, why may she not read also that which ceased to exist a century—twenty centuries since?

But, Mr. Editor, if this power exists, whatever may be the advantages that would be derived by learning in the recovery of absent or lost documents, perhaps there might be no inconsiderable drawback from the benefits of this *all-seeing* power without *sight*! Let Messieurs of the *Académie Royale*, and your friends and brethren of the healing art, your wise judges, and grave lawyers, your political agitators, your place hunters and place holders, let them ALL beware of the danger of this fearful mode of acquiring knowledge of present and passing, as well as of *past* events, by this power of reading without the sight or presence of existing documents! You, medical men, *may* have other “*correspondence*” far different from that which is carried on in barbarous Latin, and marks unintelligible to the unlearned patient, and sometimes *even* to his apothecary; nay, judges, and lawyers too, may have *notes*, which though generally of no *use* to *any* but the owners, would afford abundant *amusement* to a curious public! even divines may possess documents not necessarily preparations for sermons *only*. Now, what might be the consequence, Sir, if such curious documents should, through this miraculous power, be given to a laughing or malignant public in monthly *bulletins*? and yet what is to prevent this calamity?

But to return. In the observations I have thus submitted to you, Sir, I have confined myself pretty closely to the most *recent*, and certainly the most *aggravated* instance of mesmeric folly or imposture, namely, the attempt to exercise a *function* of sense without the *organ*. But the *mania* is daily spreading notwithstanding the repeated exposure of its absurdities by argument, experience, and fact. I may, therefore, be permitted to descant a few moments longer on some other features and instruments of this *pseudo* power—though the *nickel* experiment alone ought to be conclusive against it; for instance, may I not request attention to a few of the inconsistencies—of the contradictory statements—the ludicrous and incredible theories of a gentleman, who though he be in other instances a respectable practitioner, is, I think, next to Dr. Elliotson, the most forward advocate of this ephemeral delusion?

This gentleman has in No. XXXVIII. of the London Medical Gazette, June 1st, given an account of what he calls the “*Wonderful Phenomena of Magnetism* ;”—phenomena which he had the means, as he says, of “witnessing and verifying, and the verification of which *marvellous principle* Dr. Elliotson has come upon.” He in that letter enumerates the different classes of effects, or states, produced by mesmerism—the *source* of the mesmeric influence—and the *laws* of its transmission and reflection! and, he adds, mesmeric *prevision* and *clairvoyance*.

He adopts a classification of these under the heads of *paroxysms of hysteria*, *mesmeric coma*, *mesmeric somnambulism* or *delirium*, and *mesmeric trance*. The definitions or descriptions of those several states I have endeavoured to comprehend from his statements; but they are, I believe, utterly *incomprehensible* but by the *initiated*. For the present I omit observation on all of them except the *mesmeric trance*, which I advert to as a specimen of palpable and irreconcilable contradiction that occurs in his naked statement of this thoroughly irrational subject!

“In the two O’Keys,” he tells us, “the *trance* is brought

on when the patients are in the state of *coma*, by *rubbing* the eyebrows ; when in a state of *delirium*, by using *half* the manipulation necessary to induce *coma*. The condition now under consideration (the *trance*) was brought on by *half-waking* her from *coma*." So far for *description* of the means to produce one of the "*most important states* in which mesmerism is alleged to place the patient." If your readers, Sir, can derive any *precise* information from this statement, they have more sagacity than I pretend to possess. It is, however, to the *contradiction* which follows that I would direct your particular attention. Mr. M. thus proceeds :

"The *trance* is the most important in every point of view of the different conditions into which the patient can be thrown by mesmerism.

"In the first place, it is the condition in which, if the experiment were ever justifiable, &c. a *surgical operation* should be performed, the patient consenting to *its performance* ; the performance of which *of course would not be FELT by him*."

Now hear what Mr. Mayo, who has just described the *trance* as a condition in which a surgical operation, the amputation of a leg or an arm, may be performed without the *pain* of the operation being *felt* ; hear, I pray, what he says within three lines after the above sentence : "The *patient* so ENTRANCED *is in the HIGHEST DEGREE OF SUSCEPTIBILITY !*" and as if to demonstrate the degree of this *susceptibility*, he proceeds to say, that "if, when in the *trance*, she be placed in a chair opposite a blank wall, and no one within several feet of her, *movements* directed towards her by any one at such a distance, are followed by *sensible effects upon her !*"

But pass we now from this contradictory and irreconcilable statement to another diversity not less edifying. What does the reader suppose may be the ingenuity of those adepts in amusing the patient whom their manipulations have put into this *trance* or *coma* ? He will scarcely believe that it is neither more nor less than "*rubbing the eyebrows* if blowing upon them should

fail!" Certainly there cannot be devised a more efficient or or natural mode to arouse from sleep, *coma*, or *trance*, than thus *rubbing* the identical and proper *organ*, may I not say, of sleep! "Elizabeth O'Key in her *trance*," however it seems, "has declared that she *could* be awoke by other equally simple means or *manipulations*, such as *pressing*" (perhaps *pinching* or *pulling*) "both her ears! This she has said had *succeeded* with her," (wonderful!) "but that, however, she could *only* be awakened by *one* person, and so it turned out." The Doctor, we regret, has not told us whether that *one* person was or was not *the very one* who has had the *management* of *her*—of the experiments—and of the whole of the "wonderful process!" by which some of the public have been amused, some deceived, and not a few disgusted!

I shall not trouble you by *animadverting* on the *prevision* of Miss O'Key, who predicted on one occasion, "that in eighty-four hours she would be taken ill with a pain in the side." But she *nearly* kept her word, (we are told,) and was actually taken ill accordingly within an hour of the time predicted; and the Doctor, Mr. M. tells us, observed "it was a good hit!"

Mr. Mayo philosophizes on this incident, and informs us that this *prevision* may be *explicable* on either of two suppositions, (of which, he is candid enough to say "it is difficult to decide which is the *most monstrous one*,) 1st, that in this highly excited state of the nervous system, the *imagination* may have the *power of producing the morbid states* on which it happens to run; and that she (the imaginator) first gives her imaginings as prophecies, and then *unconsciously determines* their fulfilment!" Wonderful sagacity this in Mr. Mayo! It, however, leaves the question quite free to common sense to attribute this mesmeric prevision to the *fraud* to which assuredly common sense *will place* it. "2nd. The *other hypothesis* is," (and here the Doctor's credulity recurs,) "that in the *highly excited state of the nerves*, the *patient actually foresees* what is going to happen to her health." Here then the prophetic spirit is the gift not of

mesmerism, but simply of an “ *high excitement of the nerves*,” a state in which, I fear, every lady may be placed who is susceptible either of love or jealousy, and thus gives ample reason to hope that we may, therefore, shortly be more than usually blest by a superabundant increase of *prophetic vision*! It depends entirely on the *nervous system*—the *excitement* of the ladies!

Mr. Mayo has favoured the world with a subsequent publication, (see *Medical Gazette*, August, 1838,) in which he not only amplifies on the *previsions* of Miss O’Key, and how she one day “predicted that on another day she would assume a *new character*,” (happy for her were it so!) “and would then cease to be *without common bodily feeling*, as *hitherto had been her state* in the mesmeric delirium.” (We had not been previously informed that she had been utterly ignorant at any time of her *having a body*: as to *soul*, possibly she might have been so.) “On the morning of the 13th of June,” he tells us, “she was found to be *insensible to touch* in delirium. On the morning of the 14th June, she became again delirious, and” (again wonderful!) “she then evidently *had feeling as in her natural state*, and since presented *common sensibility*!”

But the learned Doctor proceeds—and now comes the ultimate wonder, the master-miracle of mesmerism, ‘*Transposition of Sensation*!’” to which class, indeed, belonged the experiment of reading without sight. It may be necessary perhaps, as this phrase is not *per se* quite intelligible, to inform our reader, that “transposition of sensation” means, as far as I am capable of comprehending the mystery, a power by which *every part* of the person may acquire at once, *simul ac semel*, the power of performing the function of *every* sense successively, and of *all the* senses conjointly; that is, that you may *see, hear, taste, and smell*, &c., with the tip of your finger, your *elbow*, or the *great toe*, as clearly, efficiently, and comfortably, as if the eye, ear, &c., were performing their respective functions by those natural organs in their natural position!

Mr. Mayo, in treating of this abstruse, most sublime, and miraculous faculty, quality, gift, or whatever word may best express such a miracle, assumes a character much beyond that of the mere mechanical *mesmeric manipulator*, or fogleman to the motions of Miss O'Key. He *philosophizes* most *profoundly*, and applies this very novel power of (what was formerly the RATIONAL ANIMAL) MAN, consisting of a body, with *senses*, wisely appropriated to particular organs in the human frame, with a *brain* supposed to be the seat of thought and of consciousness, and evincing *a soul*, whether material or immaterial; and he *theorizes* that from this "transposition of sensation," arises a new "physiological proof" that the soul must be "*immaterial*," contrary to what he hints is the prevailing opinion among the medical profession, or "physiological reasoners," hitherto. And the process of this new proof of the soul's immateriality, as I understand his argument, is this:

He assumes that the *soul* and the *senses* are *identical*; and he also assumes that the *senses* are as alert and nimble as in the "transposition" theory they are taken to be. Then having thus assumed the two main facts, he proceeds, as I understand him, to reason thus:

If the senses are thus capable of such inconceivable rapidity of flight (transposition) from *spot* to *spot*, on the surface of *the human body*, (equal indeed to that of a little troublesome creature of the insect class, exceedingly difficult to be followed and caught from the nimbleness and frequency of its leaps, the name of which in your learned language I know not, but which *vulgo dictum* flea), the *soul* must be *immaterial*, for if it were a *material* substance it would *necessarily* be too *ponderous* for much rapid motion.—*q. e. d.*

But, Sir, lest I should do wrong to Mr. Mayo's philosophy, or in any the slightest degree misrepresent his metaphysical reasonings on the nature of the soul, I earnestly entreat you to let me give it in his own words, with now and then a sentence or two expressive of my construction of his language.

Towards the conclusion of his letter in the *Medical Ga-*

zette of August, 1838, p. 774, Mr. Mayo, after the passage in which he mentions the “marvellous phenomenon of *transposition of sensation*,” proceeds as follows :

“ I have said that I consider the principal interest of these strange facts to be *psychical* (to distinguish from *physiological*.) I venture to put the following interpretation upon them.

“ Hitherto the tendency of physiological science has inclined towards *materialism*. Every new discovery regarding the nervous system up to the present time, has tended only to shew more and more the dependence of mind on the bodily organization, by parcelling out and assigning” (this I take to be a sly hit at *phrenology* !) “ separate mental operations to separate parts of the frame and nerves. And, however by reasoning drawn from other sources” (than physiology, I presume,) “ *one has* shaken off or struggled against the weight of the physiological argument, *one has* always felt its influence in straitening” (*narrowing*, I suppose, or *shutting up*) “ those chinks and openings through which *one has* caught glimpses of *spiritual*” (*immaterial*, he means) “ existence, and its force in habituating us to attribute more and more influence and *compulsion*” (I cannot even *guess* what the learned psychologist means by *this* word) “ to matter. But the phenomena which I now mention that I have witnessed, and which are already admitted as *part of nature*,” (by *nature*, here, he must understand all that exists material and immaterial, *that* has actually *taken place*, or is *reported* to have so,) “ by some of the first surgeons and physicians of France and Germany, seem directly to lead to other conclusions,” (i. e. the *immateriality* of mind,) “ and to support those convictions which we derive from a *higher philosophy*,” (viz. the immateriality of the soul proved by mesmerism!) “ I think that the phenomena of *prevision* and *transposition of sensation*,” (and those of *clairvoyance* likewise, which Mr. Mayo adopts by believing the acquisition of *clear* knowledge of all surrounding objects without the aid of *any* sense,

and of course, the power of reading without vision! if the latter are true, and there is anything in them beyond the workings of an over-active imagination) “naturally lead to the supposition that they result from the workings of a *spiritual* nature in a *certain* independence of those bodily organs to which it is *normally*” (I am utterly at a loss to divine what use he makes of this word here) “*tied and bound.*”

The whole of this passage clearly indicates his opinion, that the *spiritual* immaterial soul is perpetually disporting itself and gossiping about the visible and invisible world, distinct from and independent of body, and that it *hears, sees, touches, &c.* without troubling what we call the organs of sense, &c. He then proceeds: “It is conceivable that in such cases as I have described, when ALL the COMMON avenues of SENSATION” (that is, all the *organs* of sense) “are secluded,” (I suppose he means *blocked up*,) “when eye-sight, taste, touch, hearing, are suspended,” (i. e. the organs laid by as useless) “and when a *sort* of *vision* is *sensibly* exercised, (*sort* of vision? oh! by *some* part of the *common surface of the body*, the *hip*, for instance, or the *nose*, or the *lower part* of the spine for the *eye*; the *callus of the heel*, or the *toe nail*, for the *touch*!) that these phenomena arise from the MIND,” (i. e. the *spiritual, immaterial* essence,) “being in part *dislocated*” (the *part* of a spiritual being!) “and *displaced* from her *corporeal* tenement,” (a tenement which, I presume, is to be let *unfurnished*, while the vagrant mind is *rambling* up and down abroad, unattended by any one organ of sense, and freed from all trouble of housekeeping!) “holding on with *misplaced* attributes,” (this *displaced* mind with *misplaced* attributes I admit is beautiful!) “to unaccustomed *points* and *corners* of the frame.” (I suppose this means parts or points of the surface of the body where the *mind* sits in a freakish state, distant from her proper but deserted home!)

Mr. Mayo proceeds: “It is conceivable” (not by *me*,

certainly) “that in that wrapt and mysterious state” (*vagrant* he should have said) “in which the individual” (i. e. for example, the body and mind of Miss O’Key) “is giving utterance to remote anticipations,” (he must, I think, have written ‘anticipations of remote events,’) “that are” (shall be hereafter) “strangely verified; the mind is acting *independently of its usual*” (but *quite superfluous*) “*organs*, and with the character of spirituality” (i. e. *immateriality*) “is freed from the restraints of time, as in *clairvoyance*, (if that state ever exist) it would appear to be free from the restraints of space!” “Man, we are told, was made in the image of God; *these*” (i. e. the manifold tricks and *previsions* of Miss O’Key!) “may be partial revealings of the *parity* of the spiritual nature of the *created* being to that of his *Creator*!” (Can he possibly have meant any thing so ludicrous and so impious as to use the word *parity* here for ‘equality’ with, or similarity to the *Deity*?)

Mr. Mayo proceeds to refer shortly to *tradition* and *popular* belief, as supporting the hypothesis he has thus advanced; and to Homer, Shakspeare, and Scott, great “*knowers*,” he says, of human nature, who have represented the prophetic spirit as occasionally manifesting itself immediately before the approach of death, when “*it may be supposed that the soul is loosening herself from her corporeal*” (and *long deserted*!) “*residence*.” He concludes by a “perhaps that in the origin of such superstitions there may be something in common with the wonders of mesmerism!”

Such, Sir, is the *mesmeric philosophy*!!! I now, after too long a trespass on you and your readers, leave it to the *justice or mercy* of them and of the public!

ANTI-QUACK.

ART. XIII.—*Some Remarks on the Hooping Cough, communicated in a Letter to Dr. R. J. Graves.* By DR. H. C. LOMBARD, of Geneva.

MY DEAR DR. GRAVES,

I resume my long interrupted medical communication with you by some remarks on the hooping cough, which has been so prevalent at Geneva during the last winter and spring. Seldom have I seen so many children attacked at the same time with the convulsive cough; and my field of observation on the sufferers under this painful complaint has been proportionably great. My remarks have been made on the symptoms, the duration, and the various complications of the hooping cough, as well as on the mortality, causes, and treatment of this disease. You will see hereafter, that I have come to some practical results which I am glad to communicate to you, who take so much interest in the means of lessening the sufferings of your fellow-creatures.

The symptoms of the hooping cough have been the same in Geneva as elsewhere; it has generally begun in a slight catarrhal affection, with a short, dry cough, which instead of becoming less and less, increased from day to day, and after a period of two or three weeks became quite convulsive. This first uncharacterized period has sometimes lasted six or eight weeks, and sometimes has been totally wanting, so that the patients had in the space of a few days the convulsive cough; but the most general occurrence was a short, dry cough, for two or three weeks, and afterwards a regular fit or paroxysm recurring from ten to fifty times a day. The fit or paroxysm was most generally divided into two distinct parts, with an interval, during which the patient could breathe easier, and have a few seconds or minutes of rest. It was generally preceded by a great state of anxiety, which lasted from five minutes to half an hour; the little patients used then to cry, and were very much agitated:

older patients used to announce their fit a long time before its appearance, and they were troubled sometimes with a difficulty of breathing and sometimes with nausea. In a young girl aged seven years, the nausea was so intense before the fit, and lasted so long, that the only period of rest which she enjoyed, was that which followed the fit, and even that was of short duration. Sometimes, however, those precursory symptoms used to vanish, and were not constantly followed with a regular fit of hooping cough; and I have chiefly remarked this favourable result when the patient's attention was directed to some interesting object, or attracted by conversation. The mucus expectorated was generally viscid, whitish, and transparent; sometimes, however, I have found it in the latter period yellowish and even greenish. I have not met with many cases of swelling of the face in the most violent attacks, and I have reason to think that this symptom, which has been considered as constant in the hooping cough, is by no means a usual attendant of this disease; and the more so, as I have seen during the last winter two cases of simple catarrhal affections in children, who had the swelled appearance of the face which has been considered as characteristic of the hooping cough. I have often met with profuse hemorrhages from the nose; but they have never been attended with any danger, on the contrary, they seemed to relieve rather than to increase the violence of the symptoms.

Nausea and vomiting, chiefly the last, were amongst the most constant symptoms observed in my little patients; some of them have seemed for weeks to have thrown up all that they had swallowed, and yet they were not much emaciated; so that I am disposed to think that the very effort of vomiting presses down the pylorus a certain part of the food, and gives an aliment to the absorbents. This supposition appears to me corroborated by what happens in pregnant women, who during many months seem to vomit all they have swallowed, and yet are not much emaciated, at least not so much as they should be were they to retain no food for the process of nutrition. The tongue

has generally been white and furred in most cases, and yet the little patients have generally longed for food, and taken it with great pleasure. The bowels have generally been as regular as they are in children of the same age and constitution.

The progress and duration of the hooping cough has been quite different in most of my little patients. The average duration has been from seven to eight weeks; in some cases it did not exceed three or four weeks, but in other cases it has been protracted to as many months. I have heard of cases which had lasted a year or even eighteen months; but none of those cases have come within my personal observation, and I entertain much doubt as to the correctness of this fact. I have not found weak or debilitated children more subject to violent fits of the hooping cough, than strong and healthy boys or girls; and the only cause of an increased cough has been the presence of many patients in the same apartment; and I think it would be a prudent line of conduct to put the little patients in different rooms, otherwise when one has a relapse, all the other follow at a short interval, and in this way the duration of the complaint is very much increased.

The progress of the hooping cough has been very irregular: the first period has sometimes been entirely wanting, at other times it has been much protracted; the second period, during which the cough had attained its acme, has been often very short, while the period of decrease has lasted a very long time. In most of my patients I have observed a temporary return of the cough, which seemed to return with as much violence as before; but this relapse, though frequent, was never of much duration; and after two or three days the regular decrease continued its course. The fits of cough were observed day and night; and at first I had not perceived whether they were more frequent during the day or during the night; however, having investigated the subject more closely, I have come to the following conclusion: during the period of the increasing cough, the fits are more frequent at night, and when the hooping

cough is on the decrease, the fits are more frequent during the day. The following table shows this fact as observed in two patients, aged, one two years, and the other four years :

			NUMBER OF FITS.		
			Day.	Night.	Total.
CASE I.—1st period, (from the 27th January to the 13th February), . . .			134	143	277
2nd period, (from the 14th February to the 4th March), . . .			148	123	271
			<hr/> 282	<hr/> 266	<hr/> 548
CASE II.—1st period, (from the 27th January to the 13th February), . . .			231	288	519
2nd period, (from the 14th February to the 4th March), . . .			152	126	278
			<hr/> 383	<hr/> 414	<hr/> 797

In both cases the greater number of fits is during the first period at night and in the second period during the day time. I have met with many instances similar to this, and since I have communicated this singular result to other practitioners, they have had many opportunities of confirming its correctness ; and amongst other examples, I may name the two sons of one of our most distinguished surgeons, who told me that he had made the same remark in his family. The fact being well ascertained, what is the explanation ? It is easily understood, that during the the first period so violent is the cough that it awakens the patient from the soundest sleep ; while when the fits are weaker, the frequency of the cough is not so great as to awake the patient. This supposition, however, explains only part of the fact, as it leaves undecided why the greater number of fits takes place in the night during the first period, when the agitation and cries of the little patients seems to bring on constantly new fits. So we are led to suppose, that notwithstanding this last cause of increased cough, it is very likely that motion, amusement, and

the open air, contribute to lessen the cough; while rest, the horizontal posture, and the close air of sleeping-rooms, increase the tendency to the return of convulsive cough. The last conclusion is the more striking, that in adults and in other cases the cough is rather diminished than increased under the last-mentioned circumstances.

I have not met with a single well authenticated case of secondary hooping cough; and what may have induced some authors to admit such cases of relapse, is the peculiar prevalence of a convulsive cough amongst the adults, while the hooping cough exists in the same town. During the last winter we have seen many persons attacked with violent fits of cough, attended with vomiting, tears in the eye, and bleeding from the nose. But most of those cases were such as to preclude all idea of a true hooping cough, and in most occurrences it happened in persons who had no communication with children labouring under the convulsive cough.

The only cause which I can admit for the hooping cough, is its transmission by contagion; the proofs of this assertion are the following. In most families where one child was attacked, all the others followed at a short interval. In the schools the transmission has been rapid and general; and in the town of Geneva we have traced the first cases as having caught the hooping cough in a neighbouring town, where it had been introduced by a sick child arrived from another country. The only exceptions to the transmission of the hooping cough to members of the same family, has been observed on infants who were suckled; and it is a popular opinion in the United States, that infants at the breast will not catch the hooping cough. I was attending, last winter, an American family consisting of five children, who were all attacked with the convulsive cough except the youngest, who was not weaned till the complaint had entirely subsided in the family.

The various complications of the hooping cough well deserve the attention of the practitioner, as many are serious

enough to prove fatal, and others lengthen the complaint, and make it last many weeks, even months. I have never seen a single case of the hooping cough becoming a dangerous complaint when no complication was to be met with, so that I may safely assert, that the hooping cough never ends in death unless attended with some other disease. The various forms of complications which I have observed are, bronchitis, pneumonia, anasarca, water in the brain, remittent fever, and a disordered state of the stomach and bowels.

The inflammatory state of the bronchia is a frequent complication of the hooping cough ; it is generally attended with a short cough between the fits, and with much fever and agitation ; there is generally much uneasiness before and after the fits, and this uneasiness is caused by the difficulty of breathing, and pain felt in the chest. The bronchial inflammation runs easily into pneumonia, and both are frequent complications of the hooping cough. Most of those cases that terminate in death, are attended with inflammation of the lungs ; however this is chiefly to be met with in children who are not properly taken care of ; and in the higher ranks of society I have not seen one single instance of this case of death ; indeed, so great is the difference in the mortality of the various ranks of society in consequence of the hooping cough, that I may fairly assert, that out of ten fatal cases, nine belong to the poorer classes. I have seen this difference in my own practice, which since the last four years has become more respectable, and the consequence has been, that while in 1833 I had lost four patients, in 1838 I have not lost one, though my little patients have been twice or three times more numerous ; but they, almost without exception, belonged to the higher classes. Anasarca is one of the frequent complications of the hooping cough. In most cases there is a slight degree of œdema on the face and arms, but in some more serious occurrences the serous effusion in the cellular tissue and in the cavities extends to such a degree as to cause

death. I have not met with such cases in my own practice, but another physician of this town has described to me three cases which have proved fatal, with symptoms exactly similar to those of the dropsy which follows scarlet fever, and in none of the three patients had this complaint been observed, or likely to have taken place.

Water in the brain is one of the most serious complications of hooping cough, and it is not a rare one. The cause of hydrocephalus is easily found in the constant trouble of the circulation during the spasmodic fits of cough ; the face becomes then purple, the nose bleeds, and all the veins are swelled to such a point that they seem likely to burst ; this intermittent stoppage in the brain circulation is a frequent cause of hydrocephalus in children labouring under the hooping cough. But besides the above mechanical cause, there is also some great disposition to serous effusion of the ventricles which may depend upon the nature of the convulsive cough ; this complaint has undoubtedly its seat in the origin of the nerves, and consequently the brain is originally affected ; so that it is not to be wondered at if it induces so often the formation of water in the brain. The only difference which I have been able to trace between the spontaneous hydrocephalus and that which comes in the course of the hooping cough, is the different state of the bowels, which are not so costive in the last as in the first. But a greater number of facts is necessary to make it a general rule.

I have often met, in patients labouring under the hooping cough, with a continued or remittent fever ; it was sometimes attended with shivering, hot skin, and night perspirations, so as to resemble consumption. Sometimes the fever is constant, and lasts for days and weeks. I have seen such cases where it was impossible to find the cause of the continuation of fever. The chest, examined with the greatest care, did not show any inflammation of the lungs or of the heart ; the stomach and

bowels were in very good order, and after the strictest search, I was obliged to consider this frequency of the pulse and heat of the skin, as caused by some obscure local inflammation. So serious was one of these cases that it terminated fatally, and unfortunately I could not obtain permission to examine the body.

The stomach and bowels are generally in good order during the hooping cough; but in some rare cases I have met with a loaded tongue, nausea, and loss of appetite, and in such occurrence gentle aperients succeeded in doing away with this complication. Often have I seen diarrhoea, but it has never been of much consequence, though sometimes attended with fever.

Of the two epidemics that we have had lately, the first has been the most dangerous for children. From August, 1833, to March, 1834, *twenty-eight* children have fallen victims to the complications of the hooping cough. While, from August, 1837, to March, 1838, *twelve* only have died in consequence of the same complaint. The forty cases of death have taken place in the following months :

In 1833 and 1834.		In 1837 and 1838.		Total No.
August,	3	1 4
Sept.	9	1 10
October,	8	0 8
Nov.	3	1 4
Dec.	3	0 3
Jan.	1	5 6
Feb.	0	3 3
March	1	1 2
	<hr/>		<hr/>	<hr/>
	28		21	40

the first period, the greater number of fatal cases was in autumn, while in the second, it was in winter; but the cases have been too few to draw any practical inference respecting the best or worst seasons in cases of hooping cough.

The age of the forty fatal cases was the following :

Under 6 Months,	6	}	13
From 6 to 12 Months,	7		
From 1 to 2 Years,	.	.	10
From 2 to 3,	.	.	6
From 3 to 4,	.	.	7
From 4 to 5,	.	.	2
From 5 to 6,	.	.	2
Above 6,	.	.	0
			—
			40

The above table leads us to a very important fact, viz. the danger of the hooping cough in infants, and the decreasing mortality of this complaint as children grow older ; to those above six years not one case proved fatal. We may draw the conclusions, that the danger of hooping cough is in inverse ratio with the age of the little patient. This result is at variance with the general opinion amongst English practitioners, who think that in the seventh year, and in children above seven years, this complaint is attended with much danger. If other facts concur with the above, it will be an additional proof to the usefulness of medical statistics, or rather of the substitution of direct observations to the vague experience of practitioners, who have kept no record of their cases, and who, however, pretend to draw inferences from vague recollections.

The treatment of hooping cough has, at all times, much occupied the attention of the profession, but so various have been the results of experience, that each practitioner advises a remedy as far superior to the practice followed by others ; and so much has this path been followed, that we have now a long list of unfailing specifics which have done wonders in the hands of their inventors. And yet, after all, I come to advise a new treatment for this complaint. I have also my specific, and I give it to the public with as much confidence as any of my predecessors. However, I am not so exclusive as to have tried

only one method, and I will give the result of my experience on those which I have followed with care and attention.

Emetics are rather a matter of course than of choice in infants and children, who, being unable to expectorate, swallow all the mucous formed in the bronchia ; it is also necessary to give a certain activity to the expiratory muscles, and in that also emetics are useful. I have given these every day, or every other day, and have always found it as good to unload the chest as an aperient for the bowels. They are also useful as a preparatory measure for some remedies which act better when preceded with emetics. The syrup of ipecacuanha has been used nineteen times out of twenty ; when some particular reason prevented its administration, I have given the powder of ipecacuanha, or even tartar emetic, which, however, does not agree so well with young children as with adults.

I have made a great use of assafoetida both internally and externally. Rubbing the spine with the tincture has often been of great service, and a plaster applied on the chest has helped the actions of internal remedies. I have sometimes given assafoetida in pills, but few have been the instances of the children who could swallow this truly named *stercus diaboli*. The flowers of zinc is a very good antispasmodic in hooping cough : in 1834, I have made great use of it, in the dose of four to twelve grains a day, and I must say that I have often succeeded in making the fits less and less. I have seen complete cures accomplished with that single remedy. In two very young infants who had a convulsive cough, attended with symptoms resembling epilepsy, the oxid of zinc has proved very beneficial, and has stopped the cough and the spasmodic fits. I have never seen any bad consequence from the use of this remedy.

Opium, in various shapes, has enjoyed at all times a great favour in the treatment of hooping cough. The chief preparation which I have used is the syrup of white poppy, in the dose of a tea-spoonful once, twice, or three times a day. In some cases, it has taken away the most troublesome symptoms,

but without shortening the duration of the disease. In those cases in which it has proved beneficial it has diminished the number of night fits by making the sleep sounder; but even then it seemed to have little action on those which came during the day.

I have often given prussic acid, and in cases similar to those in which the opiates were indicated, viz. when there was much irritation, and a great variety of nervous symptoms. I have generally given half a grain, and sometimes as much as one grain of the hydriocyanuret of potash in the twenty-four hours, but I have never dared to give a larger dose of a poison like prussic acid. Employed comparatively on a brother, whose sister was taking the sub-carbonate of iron, this last remedy had a most undoubted advantage.

Belladonna, in extract or powder of the root, has very often succeeded in cases of hooping cough. I used to give from half a grain to two grains of the root, and in many cases with advantage; however, though the cough was less troublesome and the fits less numerous, yet it seemed more to act as a palliative than as a curative remedy, and in many cases it certainly failed and proved quite inefficacious.

I come now to my specific, or rather to the remedy advised by Dr. Steymann, as the best anti-spasmodic in hooping cough. Dr. Steymann had advised to give from four to ten grains of sub-carborate of iron in the twenty-four hours; he gave as a rule to increase one grain for each year, so that a child six years old was to take six grains in the day; but from the beginning I found that dose quite inadequate, and I increased it to twenty-four, and even thirty-six grains in young children. I have given it either with water and syrup or mixed with a cough mixture. It has never produced any inconvenience, on the contrary, I have found that all the children treated after this method were much less weakened, and recovered faster than with all other remedies. The proofs of the advantageous effects of the sub-carbonate of iron have been so numerous that I can scarcely enter into the

detail; however, I may give a few facts to corroborate my assertion. In a child, four years old, I gave the subcarbonate of iron, and the fits which in the preceding week had been 101 in number were reduced to 66 in the following week. In a weak and debilitated boy, aged seven years, the powder of belladonna had proved quite useless when I tried the powder of iron, so prompt was the effect, that in a few days the boy was quite cured; the sister of this boy was also cured with great rapidity. A young girl, aged eight years, had eight fits in the day, and after a fortnight they were reduced to two or three very mild fits of cough. A boy, aged six years, having thirty fits of convulsive cough during the day, when he began the subcarbonate of iron, after one week the daily number was reduced to twenty-one, and in a fortnight, to eleven or twelve fits, much less violent than they were before the treatment. One of our best apothecaries had tried various remedies on his children, who were labouring under a violent attack of hooping cough, when I advised him to try the sub-carbonate of iron; the result was far beyond his and my expectations, as after three days the night fits ceased entirely, and those which occurred during the day were reduced to three or four. The last case of hooping cough which I have treated lately was of four months' duration, and every thing had proved useless, when I gave the iron powders, which in the space of a few days succeeded in making the cough less and less.

In fact, I think I may assert with security, that the subcarbonate of iron enjoys a remarkable property to make the fits less violent, to diminish their number, and after a certain number of days to cure entirely the hooping cough. It enjoys, besides, the advantage of strengthening the little patients, and to give them the force to resist a complaint which sometimes lasts some weeks, and generally leaves the patients weak, low, and exhausted. In some of those who have taken it, I have often seen during the first days a temporary increase of the cough, but it always subsided after two or three days, and did not prevent

the good effects of the medicament. The good effects obtained by the use of the iron powders are easily explained by its anti-periodic and anti-neuralgic properties, and it shews *a posteriori*, how much the whooping cough resembles a true neuralgic, or at all events a true nervous disease.

Before concluding this long letter, I must not omit to mention the remarkable effects of a change of air; indeed, and striking is the fact, that it is now a popular remedy, and many have been the instances which have proved to me that this opinion is founded on a sound observation. In many cases which had baffled all attempts to stop the cough, a change of air has accomplished the cure. I have found it equally indifferent to go out of town or to come into town, provided there is a change; and even at the short distance of half a mile I have seen the good effects of this plan of treatment. In many cases I have remarked that during the first three or four days the change of air increased the cough, which became afterwards much less, a remark similar to that made after the use of sub-carbonate of iron.

Now, my dear Graves, let me take leave of you, and beg your pardon for having been so tedious, and let me hope that some of my opinions on this interesting subject may have met your approbation, it will be an excuse and a satisfaction to your sincere friend

H. C. LOMBARD.

ART. XIV.—*On the Occurrence of Crystals in the Human Intestines.* By O'BRYAN BELLINGHAM, M. D., Surgeon to St. Vincent's Hospital, &c.

SEVERAL accounts have appeared of concretions having been met with in the intestines of the human subject, some of which were of a very large size; these however, from the cases which

have been published, appear to have had almost altogether a vegetable origin, and usually as a nucleus had some foreign body, such as a piece of bone, a plumb, or cherry stone, &c.

It is only within the last two or three years that the occurrence of distinct and separate crystals in the fæces has been noticed, although it has been stated that Professor Ehrenberg, long ago, observed microscopical crystals in the meconium, from which he concluded that they might be found also in the intestines of adults.

The attention of the Profession was called to the subject by a communication from Professor Schoenlein, of Zurich, published in Müller's Archiv. for 1836, and noticed in the Edinburgh Medical and Surgical Journal, for July 1837; in which he states, that in an epidemic of abdominal typhus, (which prevailed at Zurich, in 1835,) he found in the intestinal discharges of a typhus patient, a great number of microscopic crystals, which were transparent, slightly fragile, consisted chiefly of phosphate of lime, some sulphate of lime, and a salt of soda, and presented the appearance of rhombs or rhombic prisms.

In a subsequent communication, Professor Schoenlein states, that he had been able to establish the presence of these crystals (in the intestinal discharges of patients labouring under abdominal typhus) so uniformly, that it might be employed as an important diagnostic mark of the disease, as he had examined the discharges in other diseases and of persons in health, without being able to detect them. The most frequent form of the crystals was a combination of the rhomb with the rectangular prism.

After the receipt of Professor Schoenlein's communication, Müller directed his attention to the subject, and states, that in the excrement of adult individuals brought to the theatre of anatomy, he has frequently after long search found scattered individual crystals in persons who died of very different diseases. The crystals were partly visible with the naked eye, others with

the microscope. Several times they were right-angled tables, once a rhombic prism, and once long four-sided prisms.

This notice, which appeared in the *Edinburgh Medical and Surgical Journal*, upwards of a year ago, does not appear to have attracted much attention, as no account has been published in any of the Journals in these countries, of crystals having been looked for or met with, either in the intestinal discharges or in the intestines of individuals dying of typhus or of other diseases.

Having recently met with a considerable number of crystals in the fæces contained in the colon of an individual who died at St. Vincent's Hospital, I considered that a short account of them might not be uninteresting, particularly as I had the advantage of an analysis by my friend Dr. Apjohn, the talented Professor of Chemistry to the Royal College of Surgeons in Ireland.

The individual in whom they occurred was a male, aged about forty, who died of pleuro-pneumonia and gastritis, having been admitted into hospital only a few days previous. On examining the intestinal canal, the contents of the colon appeared of a lighter colour than usual, and more fluid. I observed some very small bodies suspended in the fæces which on removing and rubbing between my fingers proved to be solids; on drying and examining them with a lens, I found that they were perfectly regular crystals, (although somewhat less than the third of a line in length,) they were in considerable numbers, but so very minute that it required a long time and a good deal of trouble to separate a sufficient number to allow of a chemical examination.

The colour of these crystals was white, some however were yellowish on the surface, being stained by the contents of the colon. Their figure was a slender four-sided prism, terminated by four-sided pyramids, and their edges and angles were exceedingly perfect. On analysis, Dr. Apjohn found them to

be composed of the ammoniaco-magnesian phosphate, or the triple phosphate, as it is commonly called. The colon was the only part of the intestinal tract in which I succeeded in detecting them, and there existed no ulceration in any part of the mucous membrane.

The four-sided prism does not appear to be the most common figure of the crystals of the triple phosphate ; when they occur in the urinary bladder they are usually short three-sided prisms, terminated by three or six-sided pyramids.

The composition of the crystals found by Professor Schoenlein was very different from these. Müller does not mention the composition of those which fall under his observation ; but as they were on one occasion long four-sided prisms it is not unlikely that they consisted of the ammoniaco-magnesian phosphate. This compound too, seems to form the principal ingredient in most of the concretions which have been examined from the intestines of quadrupeds, in whom they are not very uncommon. Fourcroy and Vauquelin found some from the horse to be composed of the triple phosphate ; and Dr. Marcet says, that all the specimens which he had an opportunity of examining, consisted entirely of the same salt.

The concretions to which I have alluded as having been met with in the human intestines in these countries, (which are described by Dr. Monroe in his *Morbid Anatomy*,) have been noticed only in Scotland, and consisted almost altogether of the beards of the oat (which forms so prominent a part of the food of the lower orders in that part of the United Kingdom,) matted together and cemented by animal matter, with or without a central nucleus ; some of these alvine concretions however, had a coating of crystals of the triple phosphate, others of phosphate of lime mixed with animal matter.

In a recent Number of the *Dublin Medical Journal*, Dr. Harrison has given an account of some minute prismatic crystals, detected by him on the peritoneum of subjects brought to the

dissecting room, which, on analysis by Dr. Apjohn, was found to be composed of the ammoniaco-magnesian phosphate.

But, I believe the case which I have given, is the first in which separate and regular crystals have been noticed in these countries, in the contents of the human intestines. And though much importance does not attach to an individual case, yet, as far as a single one can, it will help to prove that Dr. Schoenlein's assertion is too sweeping, viz. "that the presence of these bodies in the intestinal discharge may be considered as an important diagnostic mark of typhus fever."

ART. XV.—*Researches and Observations on Plica Polonica.*

By KAJETAN KOWALEWSKI, Member of the Dublin Medico-Chirurgical Society, &c.

I HAVE long observed that many of the ideas respecting the disease called by English writers *plica polonica*, which are entertained in this country were erroneous; and as a native of Poland, the country in which it most commonly occurs, I have felt myself called upon to correct these opinions, as far as it was in my power to do so. I have accordingly embodied in the following essay, such facts as I am acquainted with.

During the last session, I introduced the subject to the Medico Chirurgical Society, by relating two cases which occurred within the circle of my own family; and having been requested by one of the Presidents of the Society to prepare the essay for publication in the Dublin Journal of Medical Science, I now in compliance with his wish, present it to the Profession, enlarged by the addition of several cases selected from the most authentic sources, and by a statement of the conclusions which I have individually been led to adopt. I leave it to my readers to draw their own inferences from the recorded cases.

In the following cases the word *plica* is employed to express an agglutination, or a matting of the hair into one or

several small masses, so as to prevent the possibility of its being disentangled.

I shall first record some examples of the acute forms of the disease.

CASE I.—I extract this case from the work of Dr. Schlegel, who practised for a length of time in Moscow, and in 1806 published his researches on plica.* A boy who had always enjoyed good health, commenced at the age of twelve to wear the national dress of Poland. In conformity with this costume his hair was shaved except a small tuft at the crown of the head; soon after this, he began to lose his health, and suffered much from various ailments, but particularly from sore eyes, and pains in the extremities. At the age of 16, he was unable to follow any occupation, and was obliged to keep his bed continually. During summer his health usually improved, but on the approach of winter he relapsed into his former condition. In this state he lingered for three years, when he was attacked with fever, accompanied by severe pain in the sides and extremities. *The tuft left on the vertex formed a plica, at which time all his sufferings ceased.* He continued the habit of shaving his head, and several small plicated tufts were successively removed. He was again seized with a violent fever, accompanied by delirium, and lancinating pains in the chest and extremities. An abundant sweat, produced by some domestic medicine, diminished the intensity of his sufferings; but he became extremely emaciated. He was now seen for the first time by Dr. Schlegel, who found him reduced almost to a skeleton, scarcely able to breathe, and with the pulse almost imperceptible. The shaving of the head was ordered to be discontinued, and the part was enveloped in warm coverings. Extract of aconite, sarsaparilla, quinine, with polypodium, were prescribed three small blisters were applied to the head, and the warm bath was ordered every third day. Under this treatment the headach diminished, the pulse rose,

* Dictionnaire des Sciences Medicales—Art. Plique.

and the skin, formerly dry and shrivelled, recovered its natural appearance. In the course of seventeen days, the patient experienced a general feeling of formication, and every hair on the body became plicated. An adhesive matter of the consistence of honey exuded from the roots of the old plica, which had long since become dry, and now began to separate from the head. The hair which began to grow on the forehead, temples, and back part of the head, since he ceased to shave, became increased in size, and filled with a yellowish-brown matter. On the breast, in the axilla, on the pubis, and verge of the anus, the hair formed large plica, while on the arms, thighs, legs and belly, the hairs became at least six times thicker than before, but remained separate from each other. The bathing was continued; but it was found necessary to avoid wiping or rubbing the body, and only to pat it with soft linen, for the slightest traction of the hair gave him violent pains, the roots being endowed with exquisite sensibility. The hair could be cut off at any part without his being conscious of it, provided care was taken to avoid pulling. At the end of five months the plica fell off spontaneously, and the patient was restored to perfect health.

CASE II.*—A lady who had enjoyed the best health until her 46th year, and had been the mother of six healthy children, began to complain of headach referred to the forehead. The pain was confined to a small spot of the diameter of two lines, at the edge of the hair; pressure upon which used to produce convulsions, while the other parts of the head could be handled freely. She compared the sensation produced by pressure to the burning with a red-hot wire. Cold water, ice, snow, &c. were employed in vain. A most offensive odour was exhaled from the surface, so that no one, not even her husband and children, could remain with her for more than half an hour.

* Alibert, *Maladies de la Peau*. The case was communicated by Dr. Lafontaine, who resided in Poland for several years. He transmitted the case along with a portion of the plica to Alibert.

Her apartment was fumigated with the most powerful perfumes, but nothing could diminish the fetor. The urine, perspirable fluid, and menstrual flux, were inconceivably offensive. After an abundant perspiration, the skin became of a brown colour like that in leprosy, but by repeated washing its natural hue was restored. The brown matter was merely the trichomatic exudation which the hair could not contain. It was found impossible to wash out the stains on the linen produced by the menstrual flux. During this period of her illness she stayed in the country without any medical assistance ; she then resolved to go to the neighbouring city for advice. On the way she contracted a catarrhal fever, accompanied by a violent ophthalmia. She experienced no relief ; and after three months proceeded to Warsaw, where she consulted Dr. Lafontaine. As her debility was extreme, he began the treatment by attempting by means of tonics and rest to restore her health. All the phenomena indicated plica, besides, her mother and grandmother had both suffered from it. Various means were now had recourse to, to facilitate the development of the plica ; decoctions of aromatic plants were used, and in about three weeks the crisis took place by means of the hair on the head, which was extremely long and thick. A plica of enormous size was formed. During this time she had occasionally convulsions, which were treated by opium. The extreme foetor of the surface ceased in a few hours after the formation of the plica ; but the headach became again insupportable. The crisis was, doubtless, incomplete, for in the course of three weeks the convulsions and sweating of the head returned. At length an enormous fissure was formed across the compact mass of the plica, from which during the space of twelve days an extremely fetid, brown fluid exuded. An innumerable quantity of vermin appeared on the outer side of the plica ; the pediculi were destroyed by mercurial ointment ; and in order to give vent to those in the interior of the plica, three incisions

were made across the mass of the hair, and the vermin then liberated were destroyed by the same means. In the course of two days the incisions were so completely united, as to leave no trace of their original situation. The patient was perfectly convalescent in six weeks, and the ophthalmia had completely subsided. Her health was soon restored by tonics, nourishing diet, wine, and fresh air. In twelve weeks the separation of the plica from the head commenced, and it was ultimately removed.

The next case which I shall record occurred in a lady, a member of my own family, in the year 1830, and is strongly illustrative of the critical phenomena of plica.

CASE III.—A lady of rank, aged 37, born and brought up on the shores of the Vistula, of bilious but healthy constitution, dark brown hair, and the mother of several healthy children, began about two months after her last confinement to complain of lassitude, weakness, violent headaches, want of sleep and appetite; the functions of the uterus became suspended. These symptoms were soon followed by fever; she was successively under the care of two physicians, who treated her for menstrual suppression, and never suspected the existence of a latent plica. She was confined to bed for upwards of three months, when all hope of her recovery was given up. At this time the headache became so intolerable, and was accompanied by such a tenderness of the scalp, that she would not allow her hair to be combed for two days successively. The hair had previously been regularly combed and dressed. The fever was at its height: she had spent several nights without a moment's rest, when on the third morning she awoke much refreshed by quiet sleep, and almost free from headache. She then wished to have her hair combed, when to her great surprize it was found matted together into a single mass, which adhered strongly to the scalp. The plica was formed in the course of sixty hours, reckoning from the last time at which the hair had been combed. It resembled a thick and closely woven wig; but no

secretion capable of producing the agglutination could be perceived by the eye: there was no fetor nor any appearance of vermin: from this time she recovered rapidly. In six weeks the plica separated from the scalp in consequence of the growth of healthy hair beneath, and was soon after cut off. She recovered perfectly, and in about a year afterwards became the mother of a healthy and vigorous child. She has ever since enjoyed excellent health.

CASE IV.—The next example is interesting, inasmuch as it occurred in the daughter of the lady whose case has been just described, and it is remarkable, that the previous occurrence of this case did not lead to a supposition of the nature of the disease in the case of the mother, in which the plica formed one month subsequently. S — K —, aged about three months, was born with fine brown hair, three or four inches long; a well developed and healthy child: her first symptoms were soreness and inflammation of the eyes, followed by a slight fever and general derangement of health; she was observed to lie constantly on her face,* relief being always afforded by this position: medical treatment was employed without effect. In a month a plica engaging the hair of the head was formed; it felt dry, exhaled no odour, and was free from vermin. The ophthalmia soon after subsided; healthy hair grew beneath the plica, which was removed in about a month, when the child's health was perfectly restored.

None of the brothers or sisters of this child had ever plica, nor was it, previous to her mother's illness, known to have appeared in the families of her parents.

CASE V.—A Jew pedlar, of the dukedom of Posen, for many years manifested symptoms of mental derangement; the functions of the digestive organs were impaired, when suddenly

* I do not know if this symptom has been noticed by any author; I would explain it by reference to the tenderness of the scalp, the child seeking this position in order to relieve the parts from pressure. The position on the face was preserved by night as well as by day.

plica made its appearance, his general health improved, and the symptoms of mania completely disappeared.*

I shall now adduce some examples of the complications of plica with pectoral disease.

CASE VI.—A woman, aged 26, for a length of time suffered from pain of the side, cough, hæmoptysis, and palpitation. These symptoms disappeared, and she recovered her health, but after an excess in drinking ardent spirits, to which she was much addicted, the hæmoptysis returned, and tubercular cavities were, in a short time, formed in the lungs. Quinine, asses' milk, and Iceland moss, were prescribed with some benefit, but she began to experience violent headaches. A blister to the back of the head seemed to relieve her; the scalp began to perspire, and at last a large plica, occupying the whole of the head, was formed. The cough and hæmoptysis now subsided, and her health was restored; but she began to drink spirits again, and died in about six weeks from the effects of intemperance.

CASE VII.—Dr. Marcinkowski relates, on the authority of Dr. Malcz, one of the first physicians in Warsaw, the following case :

A lady, the wife of an officer, was considered to be phthisical by all the practitioners of the city. Her emaciation was extreme. Symptoms of plica appeared, and from that time the disease of the chest began to diminish, and when the plica was fully developed the pectoral disease completely subsided. At the end of a year the plica was cut off, and for six years afterwards her health continued unaffected.

CASE VIII.—The following case is interesting as occurring in a native of France, the patient had never been in Poland :

Eleanor Gaudry, aged 38, a native of Lisle, in the department of the north, of bilious and choleric constitution, and dark hair. At the age of 16 she became profligate, and lived

* Marcinkowski, Archives Generales de Medicine, 2nd Ser. tom. 3, p. 65.

with a soldier, from whom she contracted the habit of drinking ardent spirits. Her features soon began to alter, she got hæmoptysis, and severe nocturnal cough. These symptoms were followed by high fever with exacerbations towards evening. Sedative medicines diminished her sufferings, but she began to be tormented by frequent headaches. A viscid fluid with an offensive odour exuded from the roots of the hair, and notwithstanding all her care in combing, the hair became matted into one thick mass. It is remarkable, that when this mass of plica was humid the chest was much relieved, but on the contrary, when it became dry the chest suffered, and she again spat blood.* She continued her excesses in ardent spirits, and after several years of suffering was sent to the Hospital of Saint Louis, where she died with all the symptoms of consumption.*

CASE IX.—An Officer, who had served for twenty years in Poland, returned with all the symptoms of phthisis pulmonalis, and with a plica composed of a few marked tufts of extreme hardness. As it gave him no inconvenience, he carried it for a long time without thinking of cutting it, but, on the contrary, he preserved it with great care, according to the opinion commonly entertained in Poland, that the existence of the plica is beneficial to the animal economy. This officer kept his plica to the seventieth year of his age, without experiencing any other inconvenience than that of pain at the roots of the hair whenever the plica was touched.

CASE X.—Thomas Quart: this patient was aged 45, of a robust frame and a bilious constitution; his features were disagreeable, and expressive of want and suffering, and his thick, long, broad, and large eye-brows, gave him a gloomy and savage appearance. He was a drunkard, and disgustingly unclean; almost every part of his body was covered with hair. He was a son of a Polish woman by a Frenchman, and born in the neighbourhood of Warsaw. When he was three or four

* From Alibert.

years old a great quantity of scabs caused his hair to fall off, it however grew to a considerable length and formed a plica. At the age of 17 he came to France, when he received a serious wound in the head, and then, for the first time, his plicated hair caused him acute pain. Soon after his recovery he became a hermit, and his hair and beard were shaved, and he remained in solitude until he had attained the age of thirty years. His cell being destroyed he fell into extreme poverty; this irregular life and total want of cleanliness, produced the return of the plica; the pain of the head returned, and he experienced a sensation of pulling and stiffness in the head, which prevented him moving his neck. He got then his hair and beard cut, from which no bad consequences followed, and the operation was repeated three times. In consequence of a fall, he received a wound in the temporal region; he was removed into hospital, where, in order to facilitate the dressing of his wound, almost all his hair, which was black and of extreme fineness, was cut off. Nothing was left on the crown of the head but a small tuft, composed of about fifty plicated locks; these were matted by a greasy unctous, fetid matter. Their arrangement could only be compared to the texture of felt. These locks, long enough to cover a part of the face, were nearly of the thickness of the little finger; they were of various sizes, the smaller often combining to form larger ones. The manner in which the hair was matted together gave to the locks a contorted and knotty appearance, they were of a conical form. His beard was not plicated, in consequence of his attention in washing it every day, by which means, he said, he prevented the hair from becoming heated. We must suppose, however, that these ablutions were neglected, as in six months afterwards he was seen with numerous plicæ depending from his chin, and containing an innumerable quantity of vermin. He exhaled such an offensive odour that no one would lodge him. He became extremely emaciated, his voice hoarse and weak, and he expectorated without effort a quantity of purulent matter. An insatiable thirst devoured

him, and forced him to commit new excesses. His tottering feet scarcely supported him. I examined the colour of his skin which had become earthy and scorbutic. He now formed a speculation with regard to his plica : he allowed the locks of it to be cut, which he sold to the students of medicine, to whom he became an object of study and observation. I myself, bought three such locks which I preserved in my pathological museum.*

CASE XI.—The following case was related to Dr. Alibert, by the Count S. : A poor woman, born on the banks of the Vistula, was for a long time subject to a chest affection, so that she was unable to follow any fatiguing employment. At a far advanced period of life she was attacked with a remittent bilious fever, the exacerbations of which were exceedingly intense. The disease terminated by an extraordinary secretion from the scalp, or rather a critical flux established itself towards the head, which was followed by the subsidence of the old disease. She remained completely free from the oppression of the chest. Her hair became plicated, and formed a cylindrical rope, which grew so fast, that in a short time it reached the ground. The patient preserved the plica carefully, considering it a preservative from other diseases. The plica had no swelling nor any knots upon it. Of this patient's death nothing is known.

I shall now give two examples, in one of which the disease simulated venereal, and in the other was followed by a suppurating tumour of the groin.

CASE XII.—The wife of a Polish peasant, aged 20, of a robust constitution, and enjoying perfect health, was suddenly attacked with lancinating pains in the head and articulations. In a short time fetid ulcers appeared in the throat and on the schneiderian membrane ; her hands and feet were covered with scattered tubercular growths. The disease was considered to be syphilitic, and mercury, both internally and externally, was

* From Alibert.

administered, but without any relief ; on the contrary, the patient became every day worse. She was then examined by Dr. Stabel, who considered the ulcers not to be venereal, but rather connected with plica, which was hereditary in the family, and now about to develope itself. Every night the patient experienced abundant perspirations from the head, she had depraved appetite, and the nails became rough and crooked. After the use of diaphoretics, a plica of considerable length was formed ; the ulcers healed, the tubercular masses disappeared, and all other symptoms were completely removed.*

The above case is particularly interesting, when we recollect that at the period of the first appearance of syphilis in Europe, the disease was in many instances confounded with plica.

CASE XIII.—A labourer, a native of Poland, was affected for five months with swellings of the legs, dull pains through all the body, but especially the extremities, and continual headach ; after this period the hair of the scalp formed numerous small plicæ, like cords of various sizes and length. His pulse being hard and an excess of fever being observed in the evenings, sedative medicines were exhibited, but in the morning, the pulse being again natural, thirty drops of the alkaline tincture of iron, mixed with the soap of turpentine, were exhibited ; besides these remedies, decoctions of guaiacum, sorrel, licorice, sarsaparilla, and hops were used, and every fifth day he got some manna with rhubarb. After this treatment had been continued for two months, convalescence commenced, and his hair became more contorted than before. Balsams and tonics were now administered, and the matted hair cut off partially. The other symptoms were progressively diminishing when a tumor appeared in the inguinal region, accompanied by pain and fever. Emollient poultices were applied, and after some time the tumor was opened and a quantity of very

* This case is reported by Dr. Stabel, and copied by Alibert.

offensive pus evacuated. This ulcer seemed to have a sensible effect, for the hair on the head became free of the viscid secretion which produced its matting. The wound healed, and the patient recovered; but the hair on the pubis remained still plicated, and no one dared to remove it.*

This case, and the result of the stimulating treatment, and the removing of the plica, should be studied in connexion with the first and second cases which I have given from Lafontaine and Schlegel.

In the next case the premature cutting of the plica was followed by still worse consequences.

CASE XIV.—A young lady, who for a long time had laboured under plica, suffered so much from headach and vermin, that she determined to have the plica cut off. The operation was scarcely performed when a viscid reddish humor began to exude from the roots of the cut hair, which latter became so sensible, that the slightest touch upon them produced syncope. She felt so weak that she was scarcely able to walk. Balsams and lotions were used without any effect. In this state it was determined to envelope the head with several folds of cotton. From that day the patient became better. An abundant suppuration took place, which lasted for several months, and which finally put an end to all the symptoms of the disease. The discharge gradually subsided, and the patient recovered.

Alibert relates the two following cases, illustrative of the chronic form of the disease.

CASE XV.—A country girl, married at fifteen years of age, was attacked in three years afterwards with a plica, which she carried all her life. She constantly experienced general pains in the extremities, and remained the greater part of her life in bed. The sensation of cold was so grateful to her that she would not allow any warm substance to approach her. The heat of a candle was even painful to her. On the slightest elevation of

* Dissertatio inauguralis, De Plica, auctore Math. Cordaly.

the temperature she had herself carried into the coldest places. She did not drink spirituous liquors, a habit common to persons attacked with plica, but she eat bad bread and raw vegetables. She died at the age of 70 years. Dr. Flourike made a drawing of her, which has been published in several Journals. Her plica was four yards long and one hand in breadth.

CASE XVI.—Dr. Corona mentioned to Alibert that he saw a Polish pilgrim at Rome, with a plica so long that it extended from his bed to the floor. His face was pale, he had difficult respiration, and felt occasionally extreme prostration of strength. When portions of the plica were cut, a bloody fluid escaped, which so frightened the assistants that they stopped the operation.

CASES XVII. and XVIII.—Dr. Schlegel relates a case of a woman, 78 years old, who carried a plica, which, during the last sixteen years grew ten feet long, and four fingers in breadth near the head, it narrowed towards the extremity. She had another plica three feet four inches long; and a third, one foot and a half in length. The same author gives a case of a woman in Saxony who had a plica of ten feet in length, which she carried upwards of fifty-two years, when it fell off spontaneously.

CASE XIX.—Lafontaine relates the following case:—A young woman was confined with her second child; after the pains had continued for some time, and her labour was not advancing, the parts were examined, when a large plica was discovered, which obstructed the vulva so completely that it was impossible to introduce the finger. This plica had begun to grow in the fifth month of gestation, leaving but a small aperture for the passage of urine. Lafontaine proposed to remove the plica, but the patient at first hesitated. It was finally removed, and the labour terminated favourably. No bad consequences followed, and the disease of the hair did not reappear.

The following cases are adduced to shew the hereditary nature of plica, and as establishing the fact, that it may be an intra-uterine disease. The cases which have been observed were not numerous, but may be depended upon.

CASE XX.—Lafontaine states, that after a residence of twenty-five years in Poland, he only observed a single instance of an infant being born with plica. The child was born with thirty small plicæ, its mother and grandmother had laboured under the same disease, and had five brothers who had been also born with plicated hair.

CASE XXI.—Meckel relates a case of a healthy boy with fair hair, whose hair was matted together from his birth.

OBSERVATIONS.

I. The name plica, from the Latin *plicare*, to *knit together*, is derived from one of the symptoms of the disease, namely, that of the hair becoming agglutinated or matted together, so that it cannot be disentangled. In Poland the disease gets the name of koltun, a word signifying a stick or pole, from the occasional resemblance of the matted hair to a stick. This entangling, however, is not essential to the disease; for in the case which I have first reported, the hair formed plicæ in those places only where it was sufficiently long to become matted, while on the other parts of the body, although it was diseased, exuding the same morbid secretion, tender at the roots and thickened, it continued separate. When the disease attacks horses or other animals with short hair, of which there are not a few examples, the hairs continue separate. The matting of the hair appears then to result from a purely mechanical cause, namely, the length of the hair; for the seat of the disease being at the roots, if the hair be sufficiently long, it is thus matted in consequence of the peculiar secretion exuded from the bulbs. If such be the case, the form of the plica being also accidental, and depending on the length, thickness, and elasticity of the hair, does not deserve the importance attributed to it by some writers, and lately by Alibert, who divided the disease into several species, according to the form of the plica.

I do not deny that the form of the matted hair may have relation to the intensity of the preceding symptoms, for in the

the cases where the disease appears in its chronic form, the hair was not matted into one mass, but only formed small plicæ, like cords of different size and thickness ; while in the cases which we may consider as acute from the violence of the symptoms, the hair became matted into a single mass. I believe that the divisions of plica into the acute and chronic forms, will have much more practical importance than that grounded on the form of the matted hair, inasmuch as I shall show that the prognosis and principles of treatment of the two varieties are essentially different.

II In almost all the cases which I have reported, the formation of plica presents the characters of a critical termination of the disease, and is generally the last link of the chain of symptoms which precede its formation. Here the division of the disease into the chronic and acute varieties is well marked. In the acute cases we observe a long train of symptoms followed by a rapid formation of plica, immediate relief, and complete recovery ; while in the chronic cases, the symptoms which preceded the formation of plica were by no means so acute, the plica was slow in its formation and progress, and followed by scarcely any amendment of the general health.

I do not put forward this view of the critical nature of plica as one wholly original ; in the case which I have quoted from Lafontaine, that author appears to have taken a somewhat similar view ; and Marcinkowski, author of some very interesting observations on plica,* states that he did not know a single instance in which the formation of plica was not preceded by some derangement of health ; and he considers it not a disease *sui generis*, but an endemic critical phenomenon, which may occur in the course of many diseases. When we examine the symptoms of this disease, we will be led still further to adopt these views.

III. The symptoms which preceded the formation of plica,

* See Archives Generales de Medecine.

particularly its acute cases, were those of fever more or less intense, see cases 1, 2, 3, 4, 8, 11, 13, lassitude, pains in the limbs and sides, oppression and pain in the chest, and immediately before the formation of plica there was almost always intense headach and tenderness of the scalp. In the cases 6, 7, 8, 9, 10, 11 symptoms of phthisis, namely, hæmoptysis, cough, expectoration, and loss of flesh existed. In the case, No. 13, the patient, besides fever, had swelling of the extremities, and an ulcer in the groin. In the twelfth case, small ulcers formed in the throat and nose, and tubercular masses were scattered over the hands and legs. In the fifth case, the plica was preceded by mental alienation, and deranged nutritive functions; in the first, second, and fourth cases, it was accompanied by ophthalmia; and in the third, the patient was treated for amenorrhea. Plica may be developed during gestation, as also soon after parturition. These facts show, that the diagnosis of the disease is often difficult, for the symptoms vary so much, that, as has been said by some writers, the disease simulates all other diseases.

Bearing in mind this great variety of symptoms, we must adopt one of two conclusions, either that the disease is an affection *sui generis*, but capable of assuming the appearance of many other diseases, or that it is merely an endemic critical termination, liable to occur in a variety of affections. By the term endemic crisis, I would imply such a crisis as is produced by local influences forming a peculiar predisposition to it, a disposition lying dormant in the constitution until called into action by some internal or external cause. The question is not only one of nosological interest, but its solution bears strongly on the treatment of the disease, and, in my opinion, the latter view leads us to that treatment which experience has proved to have the most happy results.

The diagnosis is really of great difficulty, but circumstances occasionally exist, which may lead us to anticipate the formation of plica in any existing disease, particularly the fact of plica having previously existed in the family,—some peculiarity

in the symptoms of the affection, and the resistance of the disease to the usual mode of treatment. I should mention here, that one of the most common symptoms of plica is a depraved appetite, so as to give origin to the common saying, *sæpe sub plica, latet seu foetus seu plica*. Depraved appetite was one of the symptoms in the cases 12 and 15. The desire for spirituous liquors also is often connected with plica. I have often known of children, who had the greatest desire for ardent spirits, which completely disappeared after some time; and it is believed that the parents should indulge this appetite, particularly if the child drinks spirits without becoming intoxicated. I myself, when a child, had this propensity, and had the habit of eating bread dipped in ardent spirits without any intoxication following. After some years, the very smell of spirits became disgusting to me. It is also believed in Poland, that the condition of the toe-nails may point out the hereditary predisposition to plica; in such cases they are of darker colour than usual, and grow rather in thickness than in length. In the case No. 12, the nails became livid and crooked; and this alteration often accompanies the worst cases of plica. I may mention here in corroboration of the above statements, that two of my toe-nails still present this characteristic appearance.

HISTORY AND TOPOGRAPHICAL RELATIONS OF THE DISEASE.

IV. The plica is almost entirely confined to certain countries. It occurs in Poland, Lithuania, Russia, Hungary, Silesia, Transylvania, and Prussia. It is most frequently met with among the lower classes of society, and often among the Jews, who have been established in these countries for centuries. It is not improbable that the Slavonic races scattered through these countries are more liable to this affection. These races are by Schaffarik and other historians supposed to have proceeded from the centre of Asia, where, as related by Roderic de Fonseca, the plica exists also. This author, when travelling in Asia, was informed that individuals of certain tribes on the shores of the Ganges, were occa-

sionally attacked with plica, which they attributed to the drinking of impure water.*

In the countries of Europe just enumerated, plica attacks animals, most commonly horses, but it has been known to affect dogs, oxen, sheep, wolves and foxes ; it has never been known to occur in birds. From these facts alone, the endemic nature of the disease cannot be doubted. It is also occasionally met with in different parts of Germany ; along the Rhine, Switzerland, in Holland and Paris.

The endemic influences predisposing to plica are unknown. It is a common belief in Poland, as in India, that it is caused by drinking impure water ; and it is certain that it is most often met with on the shores of certain rivers, particularly the Vistula and Borysthenes, and in low and marshy situations.

It has been thought by some writers that plica is simply a consequence of want of cleanliness, and it is certain that the hair from neglect does occasionally become matted. But such examples have no resemblance to plica preceded by symptoms similar to those just described. In Poland this accidental matting of hair is called the false plica, but it is evidently not a disease. I admit that the formation of the true plica, so far as the mechanical condition of the hair is concerned, may be facilitated by want of cleanliness, particularly when combined with irregular habits. The case No. 10 is certainly an example of plica occurring with great neglect of cleanliness ; but in this case there were many other predisposing causes. As the opinion in question, however, is not entertained by the latest writers on plica, I think it unnecessary to bring forward further evidence against it.

* Of the existence of Plica in India, I have not got any farther information. I have seen a drawing of a Fakir, which was brought from India, and which represents the hair and beard matted into numerous small plicæ like the texture of felt. The Fakir is represented performing a religious ceremony by remaining in the same posture for a great length of time. The hair might have become matted from want of combing and being exposed to the changes of weather.

That plica is not infectious is well known by the inhabitants of the countries where it most frequently occurs. But that a hereditary predisposition to the disease exists, appears from cases No. 2, 4, 12, 20, and 21 ; and the opinion also is generally held in Poland. The disease is now diminishing in frequency, which we should chiefly attribute to the general improvement in social habits. It was formerly a national custom among the higher classes in Poland to shave the hair of the scalp, leaving but a small tuft at the top of the head. This custom, so injurious to health, is now entirely abolished. Among the peasants the habit of wearing long hair, and of covering the head for six months in the year with heavy fur caps, still exists. This custom with want of cleanliness, must, under otherwise favourable circumstances, greatly facilitate the formation of plica ; and hence the disease is more frequent among the lower classes. Such of the peasantry who become soldiers, seldom suffer from plica, a circumstance in all probability owing to the great improvement in their habits, and the fact of the hair being cut short. It is also observed, that in those parts of Russia where the custom of bathing is generally adopted, the occurrence of plica is very rare, although in the same places the disease is not unfrequent among animals. From these facts we may hope, that as civilization advances in those countries, the disease of plica will become more and more rare.

The opinion generally prevailing, that plica was introduced into Europe by the Tartars, about the latter end of the thirteenth century, is certainly erroneous. In support of this opinion, two of the most ancient historians of Poland, Dlugosz and Kromer, are quoted ; but the fact is, that they never entertained this opinion : they only state that the Tartars invaded Poland and Russia between the years 1285, and 1288, that they devastated these countries, and filled the rivers and stagnant waters with dead bodies, the exhalations from which generated a pestilential and morbid disease, but there is not a word said to indicate that this disease was plica. The fallacy of this

opinion is farther established by the fact of the non-contagious nature of plica. We must also recollect that there was no intermixture of the two nations; for history relates that the subdued were treated with the utmost contempt and cruelty, and put to death rather than taken as slaves. And it is a most interesting fact, as bearing on the antiquity of plica in Poland, that previous to the introduction of Christianity, the cutting off children's hair was forbidden by a religious ordinance, until they reached the age of seven years, when it was performed publicly, and with religious ceremonies. The history of mankind shows us, that purely hygienic precepts are often sanctioned by the authority of religion, and the custom in question may have had reference to the danger of cutting the plica prematurely.

Plica attacks individuals of all ages, and may even be an intra-uterine disease, see cases, Nos. 20, 21. All classes of society are liable to it, and it may affect the domestic and some of the wild animals. No relation has been established between the colour of the hair and the frequency of the disease, nor is it permanently affected by it. The hair when matted loses its natural elasticity and its lustre, and is brittle; but the new hair which grows in its natural state beneath the plica, is unaltered in colour, and is more luxuriant than before. The disease may attack the same person more than once, particularly in the chronic cases, as in the examples, Nos. 1, 10, 17.

V. Of the pathology of the disease but little is known. If we consider the hair alone, we find that it may be diseased upon any part of the body.* The seat of the affection seems chiefly to be at the roots, and the matting is accidental, depending on the length of the hair.

* One of the oldest writers on plica suggests that the head of Medusa might have had reference to the disease of plica. It is possible that the appearance of a plica might have first suggested the idea of the Gorgon's head. Were we disposed to indulge in a more fanciful hypothesis, we might inquire whether the springing of serpents from the blood of Medusa had any reference to the notion of a contagious plica.

The truly pathological symptoms are, the great sensibility of the roots of the hair, and the secretion from them. In all the cases which I have mentioned these symptoms invariably occurred. This viscid fluid, which is the direct cause of the matting, seems to be either the increased and altered secretion of the part, or the material which should form the nails and the hair, altered in quality and quantity. It is of a reddish brown colour, of the consistence of oil, and has in general a peculiar and disagreeable smell. It is this secretion that has been mistaken for blood, and hence the erroneous idea, that the hair in plica, admits the red globules, and bleeds when cut. Frank, Lafontaine, and Alibert, have ascertained that in many instances the bulbs become tumified, and full of a dark fetid fluid. Sedillot examined with a microscope some hair from the plica of a child seven or eight years old, and distinctly saw that the interior canal was much larger than in healthy hair taken from the same individual. And in the first case Schlegel declares, that the diseased hair was six times thicker than natural. Yet in two cases of plica, in which the most careful injection of the integuments was performed by Meckel, the fluid did not penetrate the hair.

We have seen that the hair does not become sensible in plica, it is the pulling of the roots that produces pain, as in the cases, Nos. 1 and 2, in which the hair was freely cut off without suffering to the patient.

These facts tend to shew, that the hair performs an important part in the animal economy. It is an interesting fact, that in Russia and Poland, the hair, otherwise perfectly healthy, grows with extraordinary luxuriance; this observation is true with respect to both animals and man. The case of a young lady was related to me on the best authority, the hair of whose pubis was so long, that she was able to wind it round her body. In this individual there were no symptoms whatever of plica.

TREATMENT OF THE DISEASE.

VI.—In considering this part of the subject, we must bear in mind the division of the disease into its acute and chronic forms. In the first the practitioner has two indications to fulfil, one, the treatment of the symptoms of general disease, whatever they may be, the other, the facilitating the development of the plica. In the chronic, the plica already existing, he has to consider only the best modes for its particular treatment.

In the remarkable cases, Nos. 1 and 2, the treatment of keeping the head warm, and the use of baths and diaphoretics, was adopted by Schlegel and Lafontaine, and followed by a rapid formation of plica, and speedy recovery. And in other cases similarly circumstanced the patients were much relieved. In the case, No. 13, the treatment was conducted on different principles; phenomena occurred, which in all probability might have been prevented by more judicious management. The patient, it was said, became convalescent, but the hair was more contorted than before. Balsams and tonics were then administered, and the matted hair was partially cut off. The consequence was, that a suppurating tumour was formed in the groin, which the author supposed had a derivative action, and caused the return of the healthy condition of the hair. This fact confirms the opinion, that the formation of plica is a critical phenomenon, the development of which should be assisted as far as possible. The crisis which was prevented from taking place through the hair, seems to have been effected through a different channel, yet a perfect cure did not result, for a plica was formed on the pubis, with which the physician did not dare to interfere. We shall be led to similar conclusions, by reflecting on the case, No. 14, in which the premature cutting of the plica was followed by various sufferings, and suppuration of the scalp, continuing for several months. It also shews us the impropriety of cutting the plica, before the symptoms which accompany its formation have fully subsided. In the cases

successfully treated, the plica was allowed to fall off of itself, as in the case No. I., or was not cut off until the hair underneath had grown several inches in length, when the cutting was performed without any bad consequences. When the plica is dry, and in its chronic state, it has been often cut off with impunity, yet it is generally believed in Poland, that even under these circumstances, it is dangerous to remove the plica, as the operation is supposed to lead to general derangement of health, and occasionally to rheumatism, local inflammations, apoplexy, convulsions, and paralysis. I myself do not know of any instances of this kind, and I have often heard that individuals of the peasantry, on becoming soldiers, have had their small dry plicæ removed without any unpleasant consequences.

The danger of cutting the plica prematurely, is well known to the people of all countries where it exists; by the ignorant, it is even thought that it should never be cut off, but allowed to fall off by itself. There also exists an opinion, that plica is beneficial to the animal economy. Horses with plica are much valued, being considered healthier and stronger than others. So firmly convinced are the people of the truth of these opinions, that the plica is often carried during the whole period of life, and is considered as a blessing, securing the health and guaranteeing the individual against all other diseases. Hence also, no matter what disease may attack an individual, he will commonly fancy that it must be plica, and use all possible means to promote its formation. These opinions, however, are partly founded on truth, for the experience of ages has proved that the formation of plica is a desirable crisis, followed by recovery, or at least improvement of health, and not to be prematurely interfered with.

ART. XVI.—*On the Mechanism of Bruit de Soufflet, &c.*

By D. J. CORRIGAN, M.D., Physician to Jervis-street Hospital, and Cork-street Fever Hospital; Lecturer on the Theory and Practice of Medicine at the Dublin School of Anatomy, Surgery, and Medicine, &c.

PART II.*

IN the year 1835 I read, at the meeting of the British Association, a paper on the above subject. That paper was published in the Number of the Dublin Journal of Medical Science, of November, 1836. In the year 1837, a Report of the London Committee of the British Association, on the motions and sounds of the heart, was read at Liverpool, "On the abnormal Sounds of the Heart and Arteries," and was published in the Medical Gazette of December 2nd, 1837, and copied into the Number of this Journal for January, 1838. This Report is signed by Dr. Williams and Surgeon Todd. The conclusions laid down in those papers differ in some material respects, and to the examination of the points of difference, the observations of this paper are directed. The circle within which the points to be examined are contained is now fortunately narrow, for the experiments which I detailed in 1836 have been repeated and confirmed in the Report of the London Committee, so that we now both agree in the data from which our differing conclusions have been drawn; the correctness of the respective conclusions remains therefore alone to be tested.

The similarity of some of the experiments from which the conclusions have been drawn, it is necessary, in the first place, to lay before the reader.

* For part first, vid. No. of this Journal for November, 1836.

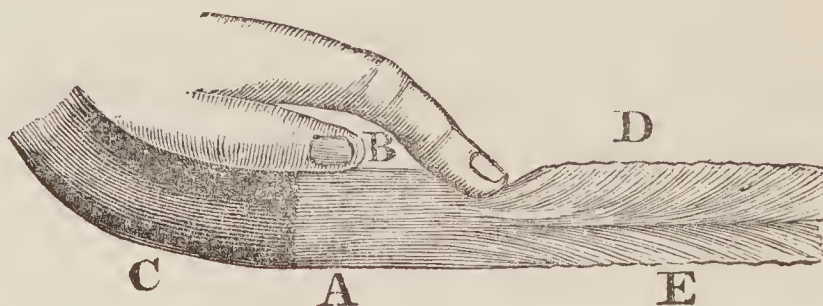
“ *On the Mechanism of Bruit de Soufflet*. By D. J. CORRIGAN, M. D. &c.—Dublin Journal of Medical Science, November, 1836.

“ The theory which is here offered of the mechanism of bruit de soufflet, can be illustrated by a very simple experiment. A portion of sheep’s intestine, or of leather or gum elastic tube, A B, is attached to the lower end of a block tin pipe, C, the upper end of which is inserted into a vessel of water, which has an elevation of four or five feet. The water is allowed to descend through the metallic pipe C, and along the flexible tube. As long as the flexible tube is kept tense, by a full supply of fluid being allowed to pass along it, the motion of the fluid as at the portion A B, is like the motion of a fluid in a fully distended artery;

“ *Second Report of the London Committee of the British Association, &c. on the Motion and Sounds of the Heart*. — Medical Gazette, December 2, 1837.

“ Exp. 1. A caoutchouc tube, 18 inches long, and 3-8ths of an inch in diameter, was attached to the stop-cock of a reservoir, in which there was water to the depth of from 8 to 10 inches.

“ When the water flowed perpendicularly through this tube (the air being first expelled,* and the lower end of the tube being kept under water in the recipient below) no sound was heard; but on pressing any part of the tube, so as to diminish its calibre, a blowing sound was heard at and below the point of pressure, and this sound became louder and more whizzing as the pressure was increased.”—p.393.



* As long as any air remains in the tube, a loud crepitation accompanies the current. In most of these experiments a flexible air tube, without its cone, was used, as being more easily applied than the common stethoscope.

it is equable and smooth, and gives out no sound, and the sides of the tube being kept in tense apposition with the contained fluid, there is no vibrating motion in them, and hence there is no fremissement in the sides of the tube. If pressure be now made by the finger upon any part of the tube, so as to diminish the supply of fluid to the portion of the tube beyond the point of pressure, then the two conditions necessary to constitute the mechanism of bruit de soufflet and fremissement are present, viz., a current-like motion of the fluid within, and a diminished tension of the sides of the tube. The sides of the tube D E become flaccid in proportion as the supply is diminished; the fluid within rushes along in irregular currents, these currents produce corresponding vibrations in the parietes of the tube, and hence over this portion of the tube, as in the experiment on the femoral artery, there is a loud bruit de soufflet, while the sides of the tube give to the finger, by their quick thrilling motion, the sensation which we call fremissement."*—pp. 191-2.

* This experiment has been related in the paper in the *Lancet* on *Bruit de Soufflet*, so far back as 1829.

Experiments on the Production of Murmurs in the Living Body.

“ If the femoral artery be pressed upon just as it passes under Poupart’s ligament, (and a short trial will enable any one to attain the exact degree of pressure), so as to lessen the calibre of the artery, and thus diminish the supply of blood passing to the portion of the artery below the point of pressure, the conditions necessary to produce *bruit de soufflet* are obtained, namely, a comparatively flaccid state, or a state of diminished tension of the artery beyond the narrowed portion, and a peculiar motion of the blood along this flaccid portion. These two conditions must be explained a little in detail,” &c.—p. 180.

“ There is an observation easily made, which confirms this view. When the temporal artery is opened to draw blood from it, if the artery be of tolerable size, and if the incision do not cut the artery quite across, then in addition to what issues from the orifice, a proportion of the blood still continues its course along the artery, rendered evident by the pulsation felt beyond the bleeding orifice. When the finger is placed on the artery beyond

“ Exp. 11. About two inches length of the common carotid artery of a young donkey was laid bare. Different degrees of pressure, either by the stethoscope, or by a probe pushed under the artery, occasioned a variety of murmurs, blowing, sawing, filing, and musical or cooing, at each pulse.”—p. 395.

“ Exp. 13. A small incision being made into the artery, a jet of blood issued, and a whizzing, sometimes continuous like the *bruit-de-diable*, sometimes only in pulses, was heard beyond the orifice, but no sound on that side of the orifice nearest the heart; the sound being, as usual, carried in the direction of the current.”—*ib.*

the orifice, fremissement is felt, while on the near side of the bleeding orifice, there is the simple pulsation of the vessel. In this instance, the state of the artery beyond the orifice is similar to the state of the large arterial trunks in the instances of excessive depletion. In the temporal artery, a portion of the blood which should pass on, is abstracted or flows out, from the orifice, and the portion of the artery beyond the orifice is not receiving the full quantity of blood required to keep it tense. The small quantity which passes on beyond the orifice no longer moves as an equable stream, but as an irregular current, and the vibrations are felt distinctly by the finger laid upon it, producing fremissement. In this instance, the opening in the side of the artery produces the same effect as pressure in the first experiment related, viz. the supply of blood is diminished in the one instance from some of it escaping; in the other from its being prevented by pressure from passing on, and the result is the same."—pp. 190-1.*

* I am sure it was only an oversight on the part of the London Committee, that in their Report they have omitted to notice that experiments so similar to theirs had been previously made by me. I had the honour of having Dr. Williams one

I have now put in apposition the principal data in which our respective papers agree, and from which, nevertheless, we have drawn contrary conclusions. These conclusions I shall first place before the reader, and then examine their respective merits. In doing this, I shall not omit to notice any of those experiments of the Committee which seem to militate against my own views.

The conclusion which the Committee consider they have established is, “that a certain resistance, or impediment to a liquid current is the essential physical cause of all murmurs produced by the motion of fluids in elastic tubes. That any condition of the walls of the tube beyond the obstructing point is not, as has been supposed, essential to the production of the sounds, &c. The more flaccid state of the portion of a tube beyond an obstruction, is a necessary effect of the impediment caused by that obstruction to the passage of water. It is therefore a necessary *concomitant* of the obstruction, but *it is not the cause of the sound*.”—p. 394.

The conclusion which I have advanced is, that “the two conditions which constitute the mechanism of *bruit de soufflet* are, 1st, a current-like motion of the blood, (instead of its natural equable movement,) tending to produce corresponding vibrations in the sides of the cavities or arteries through which it is moving; and 2ndly, a diminished tension of the parietes of the arteries or cavities themselves, in consequence of which, these parietes are easily thrown into vibrations by the irregular currents of the contained fluid, which vibrations cause in the sense of the touch “*fremissement*,” and on the sense of hearing, *bruit de soufflet*.”—p. 180.

The variance of our positions is just this, that while the London Committee *consider the resistance or impediment as*

of my auditors, and he took a part in the discussion, on the occasion of my reading the paper above referred to, and exhibiting preparations, drawings of the experiments, &c. at the meeting of the British Association in Dublin, in the year 1835.

the essential cause of bruit de soufflet, and the state or condition of the tube beyond, as in no respect necessary to the production of the sound; my position is, that the peculiar state or condition of the parietes of the tube is an essential necessary in the production of the sound, and that the sound, when heard in connexion with constriction or impediment, is only so heard when the state or condition of the parietes of the tube is such as I have described.

It is not, I think, very difficult to bring these opinions to the test, both of experiment, and of observation on the living body. If I am able to shew that the sound is not invariably present when resistance or impediment exists, and also to shew that there is no relation of proportion between the degree of resistance and the intensity of the sound, the conclusions of the London Committee must necessarily fall to the ground, and my own original conclusions remain undisturbed.

The first position I have to establish is, that resistance, impediment, or constriction may exist, and yet the sound may be absent.

The following experiment distinctly proves this: it is related in my paper already referred to, p. 193.

The farther end of a gum-elastic tube was narrowed by about one-half, by having a piece of smaller tube tied into it. A ligature was then tied round the central portion of the tube, at G, so as to diminish its calibre about one-third, and the fluid



allowed to run through as before. While the tube was filling, its sides were in vibrations, and bruit de soufflet was heard, but as soon as the tube was tensely distended, the sound totally ceased. If the resistance, narrowing, or impediment were, as the Committee assert, the efficient cause of the sound, then the sound should have continued as long as the constriction or impediment continued; but the sound does not continue, it ceases at

a certain instant, even though the constriction or impediment remains altered. This experiment then establishes my first objection to their theory, for it proves that resistance or impediment may be present and yet the sound be absent.* This experiment may be made still more striking, by applying at different intervals, on the tube, two or more constricting ligatures. There will be a loud bruit de soufflet in each of the intervals as long as the tube is filling, but as soon as the whole tube has become tensely distended, the sound ceases. For the establishment of my second objection, viz. that there is no relation of proportion between the degree of resistance or impediment, and the intensity of the sound, I shall adduce one experiment from their paper, and one from my own.

“Exp. 8.—In making the last experiment, the pressure of the water suddenly distended a portion of the tube into a globe about three inches in diameter, constituting a good representation of a circumscribed true aneurism. As long as the force of the current was sufficient to keep the walls of the dilated portion distended, no murmur was produced; but when these walls became flaccid, the passing current caused a kind of dull fremitus in them. Slight pressure on the dilatation or bending the tube to form an angle at the point, also sometimes occasioned a murmur.”—p. 394.

In this, their own experiment, a murmur was heard when the walls were flaccid, when the dilated tube being partially empty, necessarily opposed less resistance to the entering fluid, but when the force of that fluid was increased, when the walls became tensely distended, when necessarily the resistance to the entrance of the fluid was at its height, the sound ceased.

* It is necessary in this experiment to have the farther end of the tube narrowed, because if this be not done, on the ligature in the centre being applied, the portion of the tube beyond it would remain permanently flaccid, and its state or condition would then, in accordance with my own theory, give permanent bruit de soufflet. It is obvious that this narrowing at the further orifice in no way interferes with the fairness of the experiment as to the action of the constriction in the centre.

My own experiment was as follows :

“ For the intestine was substituted a leather tube, with a bulging on one side, so as to resemble an artery, with aneurismal dilatation, the discharging orifice A being smaller than the



supply tube. The fluid was allowed to flow freely through, discharging itself from the lower orifice. From the difference in size between the orifices, a short interval elapsed before the dilated tube became fully distended, and during this interval, the dilated portion could be felt thrilling under the finger, and from the same part loud *bruit de soufflet* was heard ; but the moment the cavity became tensely filled, then, although the fluid continued to flow through, all sound and *fremissement* ceased, for the sides of the cavity becoming tensely filled, they could no longer vibrate, and the sound and *fremissement* ceased.”

This last experiment is conclusive. I am glad to be able to refer to it, because, in every respect it stands confirmed by the experiment of the Committee. The conclusion from it is, I think, irresistible. The resistance or impediment to the fluid is necessarily less when the dilated portion is filling out, than when it subsequently has become tensely distended. The result inevitably then establishes my second objection, viz. : that there is no relation of proportion between the degree of resistance or impediment and the intensity of the sound.

These experiments on tubes establish, I think, those objections which are fatal to the theory of the London Committee.

There are various circumstances under which the sound is heard in the living body, for the explanation of which the theory of the Committee is, I think, quite insufficient. In *varix arterialis*, of which there is an instance given in the first part of this paper, when the arteries were pressed upon for a short time, so as

to make the tumour partially flaccid, and then the pressure being withdrawn, allowed again to fill themselves; the sound during the act of filling was very loud. In such an instance it is obvious that the resistance was less to the motion of the fluid within, in proportion as the tubes were emptied, yet it was just then the sound was loud. In permanent patency of the aorta from reticulated valves, or aneurismal dilatation of the mouth, there is no increased resistance, narrowing, or impediment to account for the sound, on the theory of the Committee, and yet the bruit de soufflet is remarkably loud. I might adduce many other instances in which I think the theory of the Committee is as inapplicable, but the above are probably sufficient. The Committee themselves seem to have felt that their theory was insufficient, for they admit that the objection may be made to their view, that "a murmur is sometimes caused where there is no impediment to the course of a liquid, as when it passes suddenly from a small into a large tube, or into a sac." Their answer is as follows:—"In the first place, it is not quite correct to assume, that in this case there is *no impediment*, for the liquid in the large tube or sac, having less velocity than that in the small one, must itself be an impediment. But besides this, the course of the small, swift current becomes changed by spreading into the larger channel, and instead of running smoothly parallel to the tube, now strikes its wall at an angle, causing a series of impulses and resistances, which, if forcible and rapid enough, constitute the vibration of sound." In reply to their explanation I must observe, in the first place, that what they state to be assumed is not assumed. It is not assumed that there is *no impediment* in the sac, but it is obvious, that before the sac is fully distended (*vid.* experiment, p. 313) the impediment is less than when the sac has become fully distended, for the fluid then found a comparatively easy passage into it, yet it was then that the sound was loud, and as the sac becomes distended, the sound diminishes, until finally, when fully distended, i. e. when the resistance or impediment is greatest, the sound is lost. Were the resistance or impediment the efficient

cause of the sound the very contrary should be the result of the experiment, the sound should scarcely be heard when the fluid is first rushing into the sac, and it should increase in loudness as the sac fills, until, when fully distended, the sound should be at its greatest pitch, and should continue so.

The second part of their explanation, viz. the irregular current-like motion of the fluid, is a repetition of the first condition which I had previously laid down as necessary to produce the sound. This condition, as the peculiar motion of the fluid within, is, however, of itself not sufficient ; if it were, the sound should continue in a sac or aneurism under all conditions of that sac. Why, in these experiments and in mine, does the sound cease when the sac is fully distended ? Their theory does not furnish an answer, but I believe mine does ; it is because when the sac is fully distended with a *non elastic fluid*, such as water or blood, the parietes are prevented from responding to or vibrating in unison with the current within, and the sound is therefore not heard. The state or condition of the parietes of the sac or tube, instead of being immaterial, as asserted by the Committee, is therefore a necessary condition in the production of the sound.

I have now only to notice the experiments, Nos. 1 and 5, to which the Committee specially refer as supporting their position, that the state or condition of the wall of the sac or tube is immaterial in the production of the sound. Experiment No. 1, is the experiment made on a tube by pressing it, so as to diminish its calibre. In this experiment the conditions which are supported in my paper are present, viz. a rippling motion of the fluid, and a flaccid state of the parietes of the tube, permitting these parietes to vibrate in unison with the motion of the fluid within, so that No. 1, proves nothing in their favour. In No. 5, a blowing sound, or a sound similar to *bruit de soufflet*, is produced by the forcible discharge of fluid from the neck of a gum-elastic bottle, although there was no continuous

tube beyond ;* and Dr. Williams, in one of his lectures in the Medical Gazette,† has repeated the same argument. “To prove this, (viz. that the condition of the walls of a tube is not material,) cut off this flaccid part, and let the tube terminate with the constricted point, you will still have the murmur produced by it.” The argument of the Committee is this, a sound similar to bruit de soufflet can be generated without the tubes at all, therefore the state or condition of the tubes is not essential to, or in any way necessary to the production of the sound. There is in this mode of argument the fallacy “*ignorationis elenchi*” of the logicians. There is a position proved which is admitted by both the disputing parties ; but that position does not overturn the conclusion against which it is directed. Their experiment proves this ; and if they inferred only so much they would be perfectly correct, viz. that the sound can be imitated or generated without tubes at all ; but when they infer from this, that the state or condition of the tubes, when present, is not essential to the production of the sound, they draw a conclusion which is a “*non sequitur*.” They might, adopting the same line of argument, as legitimately assert, that the blood within the arteries had nothing whatever to do in the production of the sound. They might imitate the sound, (and it can be imitated most closely,) by gently blowing with a pair of bellows, and then argue thus as they have already done. The sound, in this experiment of the bellows, is generated without fluid ; therefore, the motion of the blood, or in other words, its condition as to motion or rest, has nothing to do with the production of the sound. Or to show still more strikingly the fallacy of their mode of argument, suppose the same kind of reasoning applied to other investigations, as to sounds within the chest, and that the question before them was

* I was previously aware of this, for in my paper, in a note, p. 195, I have directed the discharging orifices of the tubes in which experiments are being made, to be immersed in a vessel of water, “in order to prevent the bruit generated at the termination of the tube from misleading or confusing the ear.”

† Vid. Medical Gazette, August 11, 1838.

the conditions necessary to the production of bronchophony. They would ascertain, on making some experiments, that very loud bronchophony can be heard on applying the stethoscope to the lower part of the trachea ; and the argument or conclusion on their own mode of reasoning would then be ; the sound bronchophony is heard over the trachea without the intervention of any lung : it necessarily follows, they would then say, that any state or condition of the lung, where lung intervenes, “ is not essential ” to the production of the sound. The error of such a mode of reasoning, and such a conclusion, is at once obvious, yet into this error they have fallen in the instance before us, for they have argued just in the same way. The sound *bruit de soufflet* can be produced without the intervention of any tube, therefore the state or condition of the tube, where tube intervenes, is not essential. They forget that the object of inquiry is, not in how many different ways the sound in question may be imitated ; but certain circumstances, such as blood in motion, tubes, or sacs, being given, what are the states or conditions of these under which the sound may be generated ? In the experiments on the gum elastic bottle, or on the extremity of a tube, the air around takes the place of the parietes of the tube ; that air responds to the vibrations of the rushing fluid, and the sound reaches the ear ; but if that air, the medium of conveying the sound, have this property of conveying sound lessened or destroyed, in the same proportion is the sound to be conveyed through it decreased or ultimately lost. The ringing of a bell under a glass receiver is easily heard. If the air has its vibrating power very much interfered with, by mingling with it cotton or any flocculent substance, or by any other means no matter what these may be, the sound is diminished in proportion, or ultimately lost ; so in the arteries or sacs of aneurisms, the sides of the tubes or sacs take the place, for so far, of the air. As long as these are flaccid and capable of vibrating in unison with the motion within, just as air conveys ordinary sound, so do these convey *bruit de*

soufflet: but as the sac, or tube, or artery, becomes tensely distended with the non-elastic fluid within, its capability of vibrating is destroyed, until finally, when fully distended with the fluid, it ceases altogether to be capable of vibrating, and ceases to transmit sounds. The state or condition of the parietes of the tube, sac, or artery, is therefore just as essential to the production of bruit de soufflet, as the state or condition of air is essential to the production of ordinary sound.

There is one other error of the Committee which it is necessary to notice. They say, p. 394, that “the more flaccid state of the portion of a tube beyond an obstruction, is a *necessary* effect of the impediment caused by that obstruction to the passage of water.” Such a state is not at all a *necessary* effect of constriction in a tube. A tube may be constricted in many different places, and yet all the intervals between the constricted portions be just as tensely distended as the portion of the tube above the first constriction. Some of the experiments already noticed show this, and a moment’s consideration will tell us, that provided the discharging orifice of a tube be not wider than the constricted portions, all parts of the tube, wide and constricted, will be equally distended.

The theory advanced by the London Committee had been previously advanced by Bouillaud, who, however, admitted, that his theory was insufficient to account for the production of “bruit de soufflet,” in cases where there was no narrowing or increased impediment to the motion of the blood. The question at issue between the London Committee and me, is not a mere abstract question, but one of very direct practical bearing; for it is obvious that if their Report and theory were received as correct, the student and the practitioner would be always led to associate the presence of bruit de soufflet with increased resistance, or impediment, or obstruction, and a line of practice might be adopted, the very worst adapted for many of those cases in which the sound is heard, and in which the indication very often is, not to lessen force or strength, but to maintain and increase both.

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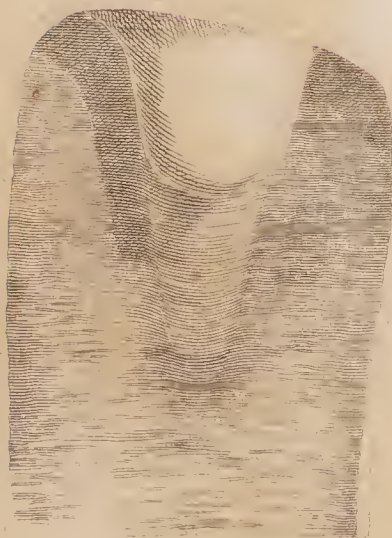
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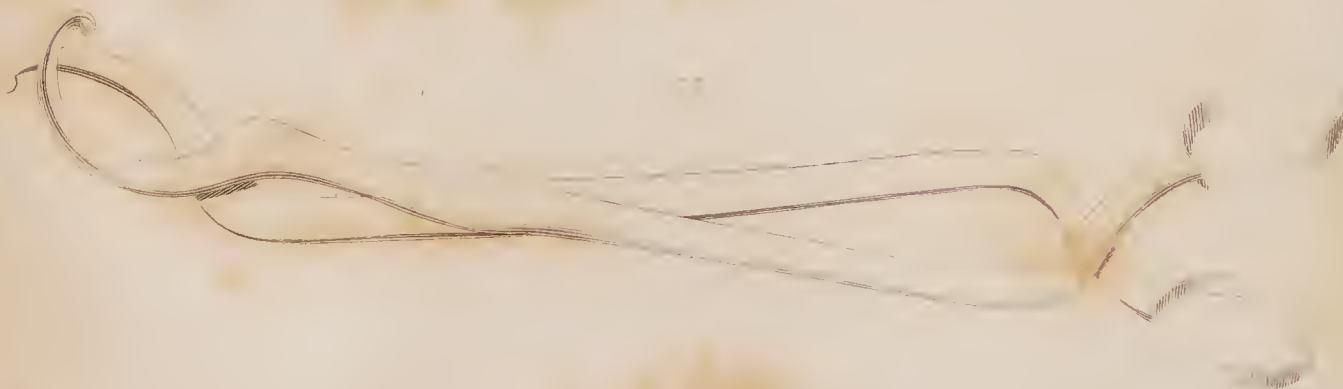
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The question at issue between us is now, as I have already observed, within a narrow circle. The experiments and observations are admitted on both sides. Their conclusion is, that “a certain resistance or impediment to a liquid current, is the essential physical cause of all murmurs produced by the motion of fluids in elastic tubes. That any condition of the walls of the tube beyond the obstructing point is not, as it has been supposed, essential to the production of these sounds, &c.”

My conclusions are, that the conditions necessary to the production of sound are—

“1st. A current-like motion of the blood (instead of its natural equable movement,) tending to produce corresponding vibrations in the sides of the cavities, or arteries through which it is moving; and

“2ndly. A diminished tension of the parietes of the arteries, or cavities themselves, in consequence of which, these parietes are easily thrown into vibration by the irregular current of the contained fluid; which vibrations cause on the sense of touch ‘fremissement,’ and on the sense of hearing, ‘bruit de soufflet.’”

The reader can, therefore, now judge for himself of the accuracy of our respective conclusions.

ART. XVII.—*Observations on Hypertrophy and other Affections of the Os Uteri.* By EVORY KENNEDY, M. D., Master of the Dublin Lying-in Hospital.

As the establishment of a ward for the diseases of females in this institution, has called our attention to a class of affections little investigated, at least on an extensive scale, in this country, in elucidating which the free use of the speculum is so essential, we feel that an apology is scarcely necessary for bringing under the notice of the professional public a treatise of this nature, as, however imperfect it may be, it is put forward simply as the result of observation, and with a view to elicit

further attention to a class of diseases, which may be considered hitherto, at least with us, in their infancy.

A clear and just definition of disease is, perhaps, more practically important than medical men are generally willing to admit. With the name of the disease is usually stamped on the mind of the physician his ideas of its nature, often of its causes, and what is more important, of its treatment and cure.

What we wish to be understood by the term hypertrophy of the os uteri here made use of, is, the existence of a *partial* and extraordinary development of the uterus, in which, not only that particular portion, but even a particular tissue is engaged. The structure deposited appears to be the existing natural structure in excess; exhibiting little morbid or diseased character, other than that depending upon increased bulk and its consequences. Writers on uterine affections treat of general hypertrophy, or hypertrophy of the body and neck of the uterus, but this disease, as far at least as our observation goes, is rarely met with. We submit that the disease to which it is our object to call the attention of the profession is, on the contrary, one of very frequent occurrence.*

The true nature of general hypertrophy of the uterus appears not to be quite understood, or at least agreed upon. Boivin and Duges, for instance, who treat very briefly on the subject, refer all enlargements, not depending on the difference of volume, observable in different persons, to *disease or chronic inflammation*.

Cruveilhier considers as hypertrophy of the uterus a state of development depending on exuberance of nutrition, in which it is invested solely with one new property, "*myotilité : d'où les efforts continuels et douloureux d'expulsion des corps étrangers.*" He instances the effect of impregnation on the uterus as the best type of this affection; and asserts, that every

* See Cruveilhier, Dic. de Med. t. x. p. 280.

strange body contained in the uterus, or developed in the substance of its walls, exerts the same effects in producing it as *conception*.

Lisfranc treats of hypertrophy as a disease attended with morbid growth, *hardening*, and fetid discharge; thus describing an affection accompanied by a train of symptoms, anything but analogous to these occurring in hypertrophy occurring in other organs. Hooper appears to take a more correct view of the nature of the disease. In considering it *simply* an inordinate or excessive development of the healthy structures of the uterus, without any morbid appearance.* With this general view of hypertrophy we must agree, not esteeming it at all imperative that any symptoms indicative of serious uterine disease should accompany it; we have at this moment in the ward for diseases of females, a case of general hypertrophy of the uterus, in which the only symptoms, in addition to the increase of bulk are retroflexion of this organ, with slight sense of weight and occasional slight obtuse pain in the uterine region.

Hypertrophy of the uterus may either affect the whole organ or a given part of it: the part most frequently engaged being the os, or to speak more correctly, one or other, or both of the lips which form the os; we shall, however, for brevity sake, treat of “Hypertrophy of the Os.” The neck may also take on this action, either in connexion with extraordinary development of the body, or with that of the os. The most remarkable case of this kind that we have met with was one of extra-uterine foetation, in which the uterus during the growth of the foetus became developed in its length (with very little increase of breadth) to the extent of twelve inches; this extraordinary growth appearing to occur principally in the neck and lower part of the body of the organ.—(See Plate, Fig. 1.) This is the case alluded to in the last edition of Burns’ work, and the preparation is at present in my Museum.

* Churchill on Diseases of Females, p. 165.

Dr. Hemmings, the intelligent translator of Boivin and Duges, describes an interesting case* of elongation of the cervix, which is evidently one of hypertrophy. Cloquet, Cruveilhier, as well as Lallemand, Bichat, and Leroux of Dijon,† have also given cases of elongation of the neck ; but those of the two former authors were combined with organic disease.

This disease has been even mistaken for polypus, and its removal has been attended with fatal results. In the hypertrophy of the neck, from the os remaining natural, with the exception of its altered position, it is, however, most likely to be mistaken for prolapsus. The detecting,‡ by a carefully conducted examination per vaginam, and if necessary by the rectum, the elongated or outstretched neck of the uterus, whilst the fundus of the organ is perceptible, of its natural size, and in its usual position in the pelvis, will sufficiently establish the diagnosis.

The affection to which we wish to attract attention is one quite different from those alluded to. In it the os itself is the part engaged. This enlargement or development sometimes occurs in connexion with elongation of the neck ; see Boivin and Duges' case ;§ but in the great majority of instances it takes place without any extraordinary development of either the body or neck of the organ ; the hypertrophy being confined generally to the lip or lips of the os.

It would seem to consist in an excessive or erratic development of the intimate or fibro-cellular struction at this part ; owing either to an inordinate action of the secernant, or an imperfect performance of the functions of the absorbent vessels of this structure. No altered or diseased action appearing to exist beyond the loss of balance in the vessels specified. This is proved by the new texture, when cut into, exhibiting all the

* Med. and Phys. Journal for Aug. 1832.

† Obs. Sur les Pertes de Sang, p. 14.

‡ See Cruveilhier's Plate,

§ Hemming's Tran. Plate 11.

characters of natural uterine structure. It possesses further this peculiarity, that the increase occurs usually in the longitudinal direction, or that of its length, not in its breadth or transverse direction, as we see in most other affections of the uterus accompanied with morbid development.

The aperture of the os ~~uteri~~, in the majority of these cases, is found in its ordinary position, bearing the same relation to the roof of the vagina as in the natural state of the parts; the development of the new growth projecting below the opening. We shall best explain the nature and varieties of the affection by giving briefly a few of the cases presented to our notice.

The case of most frequent occurrence is that in which the anterior lip of the uterus takes on this excess of development. A patient with an affection of this kind came into Hospital January 2nd, 1838, æt. thirty-two, who had borne two children; menstruation regular; had for the last eight years been subject to occasional leucorrhea, and latterly felt a tendency to the womb, as she says, descending, which was increased by laborious occupation. On examining with the speculum, the anterior lip is found protruding, and elongated to the extent of an inch and a half. (See Fig. 2.) It is free from pain, and gives the idea to the touch of consisting of the natural texture of the uterus, being neither harder nor softer: the posterior lip is of the ordinary size, the vagino-uterine aperture small, found at the base of, and posterior to, the elongated tumour.

This case was treated for a fortnight with repeated leeching to the hypertrophied part, and alteratives. The inner surface of the tumour, which was slightly granular, was touched with nitrate of silver lotion.

She left the Hospital on the 14th, reported as having the anterior lip considerably diminished in size, free from discharge, and in other respects improved.

In some instances, but more rarely, the posterior lip takes on this inordinate growth, and when this does occur, it is

seldom to so great an extent. A case of this kind was under my care some months since, in which there was, as in the former case, some tendency to prolapse. The patient sometimes suffered pain in the region of the rectum, more especially when the bowels were constipated, or on making much effort at stool. The anterior lip was healthy, and the posterior projected upwards of an inch into the vagina, but was rounded and obtuse in its shape. (See Plate 3.) This case was treated by leeching, alteratives, posture, and otherwise, as for prolapsus, and with much benefit.

It occasionally happens that both the anterior and posterior lip become hypertrophied, and this more especially in women who have borne several children. In such, the fissured or divided state of the os, that so frequently results from labour, generally continues. The anterior and posterior lips presenting separate or distinct tumours. Such a case (see Fig. 4) was under treatment, accompanied with complete procedentia of the uterus. In this patient the os uteri protruding beyond the external parts, exhibited not a bad representation of a bird with its beak separated. She was treated for prolapsus by cauterizing the vagina, a plan that I have been for some time successfully adopting, and with benefit to the procedentia, but was lost sight of before anything was done for the hypertrophy.

A more remarkable case, in which the os is fissured into three divisions, the intermediate substance becoming hypertrophied at three points, was accidentally discovered in a patient who came lately into hospital for the treatment of a vesico vaginal fistula ; both the fistula and the hypertrophy appeared to have been the result of a tedious labour in the country, where she was allowed to remain too long without assistance. The tripartite hypertrophied os is represented in Fig. 5 projecting into the vagina, but without any displacement of the uterus, or other symptom indicative of its existence. The outer surface of these three tumours is of the

natural texture of the vagina, whilst the inner is rough, of a reddish tint, with vesicles interspersed, for which she has been touched with oxymel æruginis. It may be here remarked, that the internal surface of the hypertrophied portion generally exhibits more of the furrowed, irregular character of the intra-uterine mucous membrane, whilst the outer is generally polished and smooth; the internal and most dependent portions are not unfrequently vascular, or, as in the preceding case, present slight vesicles, or granular growths may exist upon their surface.

The whole circumference* of the os may become developed without any fissured division. This is more likely to occur in females who have never borne children. For the following case of this kind, which I lately saw in consultation with Dr. O'Reilly of Dominick-street, I have to express my acknowledgments to that gentleman:—

“Mrs. —, aged 23, married eight months; subsequently to her marriage she was occasionally seized with bearing down pains, particularly while catamenia were present. These returned every ten or fourteen days with leucorrhea in the interim. Before marriage she enjoyed sound health. About three weeks previous to her arrival from England, where she resided, she suffered considerable pain from intercourse. On her arrival here she was under medical treatment until the 3rd of September, when I was sent for; she stated that her sufferings were extreme, that at eleven o'clock each day she had a paroxysm, which generally lasted two hours, accompanied with bearing down pains. Suspecting uterine derangement, she was examined *per vaginam*, and the lips of the os uteri found projecting into the vagina to the extent of an inch and a half, forming a rather conical elongation, (See Plate, Fig. 6,) the most dependent part being somewhat pointed, the anterior lip rather more

* M. Lobstein reports a case of enlargement of this description, the preparation of which is in the Strasburg Museum; in it the vaginal extremity of the neck projects three inches, containing a cavity so large as to appear like that of a second uterus. *Anat. Path. Gen.*

elongated, and its margin slightly infiltrated with serum. The os presented an appearance generally of deep red, and was painful to the touch. In consultation with Dr. Kennedy, who, at my request, saw her with me, she was treated with saline aperients and soothing opiate fomentations; leeches were applied directly to the os every alternate day, for ten days, Plumber's pill and extract of hyoscyamus administered every night in alterative doses, in addition to which absolute rest and an antiphlogistic diet were insisted on. Under this treatment she rapidly recovered, the irritability totally disappeared, and the os uteri became gradually reduced in size; she is now convalescent and able to take exercise, and since has menstruated, without suffering more than on ordinary occasions."

The disease here exhibited a mixed train of symptoms, some of the character of irritable, and even inflammatory uterus were present, evidently produced by neglect and mismanagement of the original affection before Dr. O'Reilly's seeing her. The hypertrophy here was attended with prolapsus; the patient using active exercise until it became converted from a chronic into a more acute attack.

There is a case of hypertrophy of the os uteri at present in the female disease ward, in which a curious development has occurred. In all the cases which have preceded, it will be seen that the hypertrophied portion was covered throughout its whole surface by mucous membrane, the continuation of that of the uterus or vagina. In this case, however, the fibro-cellular, or, as it is termed, the parenchymatous portion of the anterior lip, in its excess of development, has enlarged into the vesico-vaginal septum, separating the bladder and vagina, in its growth, and consequently, in place of having a distinct covering of mucous membrane, it is covered by the vaginal mucous membrane at its anterior part and point only.—(See Plate, Fig. 7.) This is peculiarly interesting, as proving the tissue that takes on the hypertrophy to be the fibro-cellular tissue of the uterus, and not the mucous membrane; the latter membrane being merely continued over it in the gradual develop-

ment of the hypertrophied structure. It is further interesting in furnishing us with a power of concluding that the disease is one quite distinct in its character and seat from that of polypus, the disease of all others which it is most likely to be confounded with polypus being a disease strictly of the mucous or glandular structure, whilst this is clearly a disease of the parenchyma of the organ itself.

From the preceding cases it will be seen that the symptoms usually attending this growth, are a sense of fulness and weight in the upper part of the vagina, which may be accompanied with heat and throbbing in this region. As the disease progresses, the patient experiencing a sensation as if a foreign body hung down into the vaginal passage; this is occasionally attended by a bearing down sensation: and in cases of long standing (particularly if injudiciously managed) may become converted into absolute prolapsus, or even procedentia of the organ. Now the practitioner may, for the first time, be aware of the existence of this preternatural growth of the os, when the error is very generally committed, of looking upon this development as the effect not the cause of the prolapsus. The preternatural growth, as well from its weight and mechanical bulk, as by the irritation it produces, acting as a foreign substance attached to the uterus. From its pressure upon the bladder, in hypertrophy of the anterior lip, interference is sometimes observed to occur with the functions of this organ: thus frequent micturition, and sometimes difficulty or extraordinary effort in discharging the contents of the bladder, occur; in other cases, particularly where the posterior lip is engaged, the functions of the rectum are interfered with, when costiveness or pain at stool occur.

On examination with the speculum, the lip of the uterus engaged is in some cases found scarcely at all altered in the appearance of its texture beyond the increased bulk; in other cases the hypertrophied portion appears red and vascular or this vascularity may be confined to a part, generally the most dependent portion of it. It is also sometimes combined with

granular disease of the os, although the menstruous secretion is seldom much interfered with. Hæmorrhagic discharges occasionally occur in this disease; and leucorrhœa is not an unfrequent accompaniment, more especially where much displacement of the uterus occurs. It is also not unfrequently productive of pain and inconvenience *in coitu*.

The throbbing, fulness, and sense of weight, may be increased about the setting in of the periodic discharge, indicating a congestive state of the part; if the speculum be now used, the hypertrophied portion many exhibit a more deep red or purple hue; from this train of symptoms the individual gets relief after the menstruous secretion has passed over. (Thus, no doubt, it has been treated as dysmennorrhœa in some cases.) The symptoms accompanying this affection at times partake more the character of irritable uterus, a disease so admirably elucidated by the late Dr. Gooch, at others, they appear to assume all those of inflammation in the tissue engaged, and sometimes they exhibit, as was the case in one of the patients mentioned, a mixed character, assuming symptoms both of irritation and inflammation, and combined with them the hypertrophied state of the os.

In these cases, where congestive or inflammatory action set in, repeated local depletion with leeches, or scarification, and the use of the warm hip bath, together with mild saline aperients, and a more protracted use of mercurial or other alteratives, combined or not with anodynes, will constitute our treatment. The washing the hypertrophied part over with strong solution of nitrate of silver also may assist in causing its absorption, or even touching it with the solid caustic, or cautery, might be necessary where there is prolapsus to a considerable extent, combined with the hypertrophy. The patient must maintain the recumbent posture as much as possible whilst she is undergoing treatment, directed to lessen the determination and sensibility in the part, and the use of internal mechanical support must be refrained from, external support being substituted if necessary: such as Hull's utero-abdomi-

nal* truss or the T bandage and pad : if these prove insufficient, then, at least, the use of pessaries should be delayed until the above treatment has been persisted in for sufficient time, and the pessary, when used, should be constructed of such a form as to relieve the parts affected as much as possible from unnecessary and injurious pressure : the ring pessary, or modifications of it, will answer best.

If the above plans of treatment completely fail, and the inconvenience arrive at a serious height from the prolapse induced or the occurrence of the other symptoms specified, then the removal of the hypertrophied portion by scissors, (see Plate, Fig. 10,) to be afterwards described, may be had recourse to.

Although any or several of the preceding symptoms may occur, hypertrophy of the os is not necessarily accompanied with any symptom indicative of its existence, as it may continue, and no doubt often does, for years, without attracting any great notice on the part of the individual so affected or becoming the subject of treatment, the tendency to prolapse possibly alone attracting attention, and even this, we have seen, may be absent.

The disease with which this affection is most likely to be confounded is polypus. Like polypus, the great extent of its surface is unattached, and its connexion with the uterus is at its upper or most distant part ; its being connected externally with the uterus will distinguish it from intra-uterine polypus, whilst its not being pediculated will serve to distinguish it in general from polypus of the neck or os uteri. Its sensibility may assist in corroborating this point, but this symptom is not much to be depended upon. It has been justly explained by Dr. C. Johnson, (Dublin Hospital Reports,) that the absence of sensibility is but an equivocal test of polypus, whilst we must state that the

* I beg here to acknowledge the courtesy of Dr. Hull in transmitting to me from Philadelphia one of these instruments, which I have tried in several cases in the hospital, in some with decided benefit. It is to be had at Messrs. Hodges and Smith, Medical Publishers, College-green.

presence of sensibility* in the hypertrophied state of the os, is just as little to be relied upon. The difficulties of deciding in some of these cases was quite felt by Boivin and Duges, as they say,† “but when it is remembered that the uterus is frequently much elongated in its descent, that its cervix projects considerably, the os uteri being perhaps defaced, its labia disfigured by swelling, and, moreover, that some polypi present on their surface depressions that might be easily mistaken for the os uteri, the value of a careful diagnosis will be readily understood.” The difficulty of distinguishing between this affection and polypus shall not, however, be always satisfactorily set at rest by the pedicular form of the stalk, as cases are met with in which tumours, reputed polypus, have grown from all the circumference of the os, and others, when one of the lips of the os appeared thickened and elongated to form the stalk. Gooch’s case, in which the removal of the tumour proved fatal, was one of the former nature. It is not impossible that these may have been rather cases of hypertrophied os than of polypus. It would appear the more likely that hypertrophy of the os uteri has been mistaken for polypus, from the opinion mentioned by some, that polypus is simply an exuberant growth of the uterine tissues.‡

The seat of the excessive growth, as already stated, constitutes a distinction in investigating these structures *post mortem*.

Hypertrophy of the lip may be confounded with prolapsus, but the position of the utero-vaginal aperture will sufficiently dis-

* This comparative insensibility has been observed not only to the touch in vaginal examinations, but also from the slight complaints made by patients in the removal of the os by the scissors, where I have had recourse to this operation.

† See “*Prac. Treat. on Dis. of Uterus,*” &c., by M. Boivin and Duges, translated and enriched with most judicious notes by Mr. Hemmings; a work which should be in the hands of every practitioner.

‡ Cruveilhier remarks:—“Je regarde comme des hypertrophies de la membrane muqueuse uterine ces regetions polypiuses pediculées pénétrées de vaisseaux sanguins, molles, spongieuses, dans l’épaisseur des-quelles, j’ai rencontré souvent des follicules muqueux.”—*Dict. de Med. et Chir.* t. 10, p. 250.

tinguish here. It is not likely to be mistaken for tuberculated uterus, as it has been justly remarked by Sir C. M. Clarke, that this disease is generally found to affect the uterus at parts remote from the os or neck, although Mr. Wenzel has certainly shown that the neck will take on similar diseased action, without even the body becoming engaged. In the class of uterine diseases termed tubercular, however, the structure is very hard, and the enlargement or development is generally in the transverse direction, whilst, as has been already shewn, in the development of disease in question, it is usually in the longitudinal direction.

A state of the uterus is sometimes observed, in which an alteration the reverse of the redundancy described is present; namely, a state of atrophy of this organ. This, as is observable in hypertrophy, may extend to the whole structure of the uterus. It is best observed in old females, particularly those who have not borne children, in whom it may be esteemed as to a certain extent a natural alteration. It is not, however, confined to these, but occurs at different periods of life; depending upon a loss of balance in the secernent and absorbent vessels, directly the reverse of that observed in hypertrophy. In this state no serious inconvenience need occur, farther than that arising from the loss of functions of the organ, in case of its getting to a degree incompatible with the performance of these. The atrophy, however, may be partial in its occurrence, affecting only a particular part or parts of the organ, when these shall be either wasted, imperfectly developed, or altogether absent. There is in my Museum, a remarkable case of this kind, in which the posterior lip of the os uteri consists of two little papillous substances about two lines in length, like the termination of an infant's uvula; the anterior lip is wanting, the surface of the vagina and anterior of the uterus being perfectly smooth and continuous. There is, however, perceptible, about half an inch down from what should be the situation of the anterior lip, a small tubercular inequality about the size of a grouse shot, under the mucous membrane; this would appear to be either the vestige of a pre-existing anterior lip, or a displaced

effort at one in its original formation. This alteration may further affect particular tissues, the fibro-cellular structure being that particularly liable to the erratic or irregular growth. We see this in diaphanous uterus, where the affection appears to depend upon absorption of this texture, the mucous and serous structure continuing in their natural integrity.

In impregnated females, hypertrophy of the os is met with much more frequently than in the unimpregnated, and this, for several reasons; first, because pregnancy, by the new and extraordinary determination which occurs to the uterine organs, would appear to furnish with the means of increased development a growth of this kind where there pre-existed such a formation in the parts; and secondly, because this state of itself is sufficient to call forth an erratic action in the organs of nutrition, even when no evidence was afforded of pre-existing hyperemia in any part of the uterus.

As the disease, from its nature, is unlikely, unless in cases attended with more urgent or complicated symptoms, such as we have described to be brought under the notice of the practitioner, its discovery in many cases shall be accidental, and of course, from the frequency of examining *per vaginam* parturient females as compared with others, it must be more frequently met with in them. Connected with impregnation it occurs as a complication of labour, when the hypertrophied portion of the os generally protrudes before the head of the child into the vagina, often puzzling the inexperienced practitioner excessively. In this way it has been mistaken for some extraordinary presentation, for placenta, for polypus, &c.

The adventitious growth of the os in impregnated females is more loose and soft in its texture than in the unimpregnated. Again, in labour this softness increases so as to give the idea of the intimate structure consisting of a pulpy mass, or even of fluid being contained in it; and this more especially

^a See Burns' Mid. p. 456.

when the labour has been of long duration. Its texture, when cut into, exhibits the ordinary fibro-cellular uterine tissue, with, particularly in cases of tedious labour, small quantities of a gelatinous fluid of a bloody tint, enclosed in some of the interstitial spaces.

The increased growth in these cases may engage the anterior, the posterior lip, or even the whole os. In the majority of cases which I have met with the anterior lip has been the seat of it. In some of these where the extent of development laterally is considerable, it spreads before the head of the child when the finger must be directed backwards, into the hollow of the sacrum, or laterally to feel the head, giving the idea of obliquity of the os.

A case of this nature occurred in Hospital in a woman named Reilly, in January last, where it became a cause of tedious labour, rendering eventually delivery with instruments necessary. Fig. 8 represents the state of the parts in this instance.

In general, however, the lip protrudes pendulous, between the pubis and head of the child, encroaching slightly upon the cavity of the pelvis; as the labour advances sinking lower and lower, until eventually it often escapes beyond the external parts. Fig. 9 represents a case of this nature occurring in a patient named Hutchinson, in No. 8 ward, in July last.

In impregnated females the hypertrophied lips vary in thickness from three or four lines to three quarters of an inch, and even more. They are polished, covered with a smooth membrane externally, but the internal surface, on approaching the os, has a rugous character. It is almost always, in labour, of a deep livid or purplish hue, and in most cases possesses little sensibility. It varies in length from half an inch to three or even four inches, and although generally broader at its base or connexion with the uterus, than at its most dependent part, yet sometimes the reverse holds, which gives very much the idea of its being a polypous growth.

Its thickness appears also to increase with the delay in the labour, especially where the hypertrophied portion be subject to much pressure, as it occasionally is from lying between the child's head and pubes. Here the part below and free from the pressure may become distended, and more smooth and tense than the rest.

A reference to the cases of this disease occurring in labour in hospital, proves, that although all labours having this complication were not necessarily tedious or difficult, yet they were so, in such proportion as to justify our classing the hypertrophied state of the os as a cause of tedious labour ; a fact naturally to be explained less upon the obstruction afforded from the bulk of the enlarged and protruding lip, than from the interference or restraint exerted by the affection upon the efficient and natural contraction of the uterus.

The practice to adopt in these cases, is to press the protruding lip very gently upwards, and retain it there with the fingers during two or three pains ; this is very easily accomplished, the tumour in general remaining, or the head appearing to be pressed down beyond it. If, on the contrary, it show any disposition to descend, then the finger may be retained for some time longer within the vagina, in order to keep the part up ; or, if this fail, a small piece of soft sponge may be pressed up, so as to rest at the upper part of the space through which the lip protrudes, and prevent its relapse. This simple manipulation we have found to act most beneficially ; the head, although perhaps resting for many hours previously without advance, coming down quickly, and the delivery being speedily accomplished. In some cases, where, from the extremely protruded state of the lips, and the want of room between the head and pubis, the tumour cannot be reduced, but rather increases in size, and the head remains without any advance, we have derived benefit from puncturing the protruding lip with a lancet: one, two, or three, slight punctures being followed by a slight discharge of blood and serum, a diminution

in the distention, and marked benefit to the progress of the labour.

In certain cases, generally those of protracted labour, one of the lips of the os (occasionally both) become infiltrated and œdematous; this œdema occurs with extraordinary rapidity. The os uteri that was perfectly natural, and of its ordinary size at the commencement of the labour, becoming, in a few hours, very much distended and pendulous. If the fluid which is infiltrated into the cellular tissue of the part be clear, no discoloration is perceptible in the tumour; on the contrary it is sometimes tinged with blood, or blood itself may be effused into the cellular tissue, constituting more an ecchymosis of this tissue. The tumour in œdema is soft and yielding to the touch, sometimes spreads before the head of the child, and evidently retards its progress. Œdema of the os is sometimes observed to exist all round the margin, without being accompanied by much increase of development of the lips; the os under such circumstances dilating generally very slowly.

In cases of œdema we must exert generally more patience, and expect somewhat protracted labour. If it appear, however, seriously to retard the labour, then two or three punctures admitting of the escape of the serum or blood, will be attended with benefit; a case of this nature occurred to us lately, in which the patient was twenty-four hours in labour before puncturing, after which the part yielded freely, the head descended rapidly, and the patient was very speedily delivered. In œdema the os may either be generally tumid and soft, with a transparent appearance of the investing membrane, and an evident distention with fluid; or one of the lips protrudes into the vagina, distended and fluctuating; where undergoing pressure, its blood-vessels becomes congested or ruptured, and blood is effused into its structure; thus increasing in bulk, and becoming of a deep or livid tint. This form which occurs so rapidly in the progress of the labour, as the result of obstructed circulation, usually, just as rapidly disappears after delivery, requiring

no treatment beyond that already specified. If it continue after delivery, then slightly stimulating or astringent injections, as of diluted camphorated spirits or alum solution, may be of service, and this failing, puncture or leeching, followed by nitrate of silver lotion, or touching with the solid nitrate, may be necessary.

The hypertrophied, as well as the œdematous and ecchymosed os, sometimes escapes the observation of the attendant till after the birth of the child, when the protrusion, either low in the vagina, or beyond the vulva, of a deep coloured fleshy mass, first attracts the attention, and excites the anxiety of the practitioner. This has been mistaken for a polypus, inversion or prolapsus of the uterus, as well as tumours of different kinds, and has even been removed under the impression of its being the first named complaint. It is to be diagnosed by introducing the fingers, or hand if necessary, into the vagina, when having ascertained that the uterus is in its natural position, by feeling the os and neck, we must satisfy ourselves that the connexion of the protruding tumour is with the neck of the uterus by an extended base, not a pedicle as in polypus. In fact, the examiner must satisfy himself of its being a continuation or elongation of the lips downwards by an excess of growth. The production is generally less tumid and thick, or rather more flaccid, just after delivery than during labour.

M. Boivin must evidently allude to this form of disease, although we are at a loss to know how she applies the term *skirrheux* to the affection, when she says, “Dans quelques cas particuliers d’affection de l’utérus son *col skirrheux* sans être augmenté en grosseur, est beaucoup plus long que dans l’état naturel son orifice externe s’avance quelque fois jusqu’à l’orifice vulvo-vaginal, et dans quelques cas même, le franchit de plusieurs lignes.”—*Mem. de l’Art des Accouch.* p. 373.

The treatment in these cases, as well as in the former, after delivery, is the same. Nature herself adopts, in most cases, a

very simple but effectual means of remedying the affection. As the uterus, but more especially its neck, contracts after delivery, the blood, which previously supplied the hypertrophied part, ceases to be sent to it, the tumour becomes, as it were, naturally constricted or pressed upon at its connexion with the neck, and thus a natural ligature is applied; the nutritious vessels being prevented sending a supply for its growth, the effect is, that a gradual but quick process of absorption goes on in the neck, and in the majority of cases the tumour will have so diminished in size, that in the course of ten days or a fortnight scarcely any appearance of it will remain. We would, therefore, recommend caution to practitioners as to the removal of those uterine growths, more especially connected with pregnancy, observed in or after delivery, as no doubt such as we have described have been removed under the impression of their being polypi; and not only an unnecessary, but, under the circumstances, perhaps dangerous operation has been performed. We should, therefore, await patiently the lapse of sufficient time from the occurrence of the labour, to allow of the absorption of the tumour before any operation be attempted for its removal. And even then, if it continue, we should not unnecessarily remove the hypertrophied portion, unless it do not yield to the treatment recommended, or inconvenience shall occur from it to a sufficient extent to justify the operation.

The delay recommended is further advisable, because operations upon the parts engaged in parturition, are attended with much greater risk in females immediately after delivery, in consequence of the predisposition then existing to attacks of hæmorrhages, hysteritis, peritonitis, &c.; and also because the longer we delay within certain bounds, the less difficulty will attend the removal of the hypertrophied portion of the organ from the diminution of its bulk, owing to the contraction of the uterus.

In its removal the risk from hæmorrhage may be guarded against by the use of the ligature; or if scissors be pre-

ferred, by applying the ligature in the first instance, and then cutting below the strictured part. This latter plan we have been for some time in the habit of adopting in the removal of polypous and other uterine and vaginal tumours, where serious hæmorrhage was apprehended. M. Wiess of London has prepared, under my directions, a pair of scissors with their blades curved, and placed at an angle with their stalks, (*see* Plate, Fig. 10,) which answer very well for this purpose.

BIBLIOGRAPHIC NOTICES.

The Transactions of the Provincial Medical and Surgical Association. Instituted 1832. Part 2. Volume VI.

WE have frequently expressed the gratification we feel at the success of the provincial schools, and provincial medical associations of England, being convinced that however great the advantages which London possesses, the system of centralization was, a few years ago, likely to produce a monopoly, the baneful effects of which would more than counterbalance those advantages. To London we still look, and must ever look, with deference and respect. The splendid hospitals of the British metropolis, the abilities of the first physicians and surgeons of the day, cannot fail to make it the *capital* of practical education, and in that capital we would advise every student to reside for a time before he commences practice. Conceding however, as is right, this superiority to London, still it by no means follows that the medical public throughout the empire should be indebted for their entire education, to that or any other city. From the working of the new Anatomy Bill, it is apparent that London cannot afford a sufficient supply of subjects, and consequently a number of anatomical schools have of necessity sprung up in other great towns, such as Bristol, Birmingham, Manchester, &c. where students may learn all the details of practical anatomy. Thus, a great number of the provincial students are, for a time at least, detained in the country, where they not only learn anatomy, but under the present system have an opportunity of studying all the other branches of the profession theoretical as well as practical. Many parents have strong objections to the removal of their sons at an early age to a great city, at a distance from their relations, and they consequently hail with joy the formation of provincial schools. The latter are every day acquiring a greater degree of character, and indeed, the reputation of our provincial brethren in England is rapidly on the increase, a circumstance partly attributable to the formation of the Provincial Medical and Surgical Association, founded by

the exertions of that excellent physician Dr. Hastings of Worcester. This Association has held many annual meetings of a most interesting nature, and has published six volumes of Transactions. The last we have read with great attention and interest, and we only regret that neither our plan of conducting this Journal, nor our limits, permit us to gratify our readers with an analysis of its contents. The following papers are contained in the sixth volume :

“ PART II. MEDICAL TOPOGRAPHY.

“ ARTICLE III.—On the Medical Topography of Exeter and the Neighbourhood, being a Sketch of the Geology, Climate, Natural Productions, and Statistics of that District. By Thomas Shapter, M.D., Physician to the Exeter Dispensary, Lying-in Charity, &c. (*With Maps.*)

“ IV.—On the Medical Topography and Statistics of Cheltenham. By D. W. Nash, Esq. of the Bengal Medical Staff. (*With Maps.*)

“ PART III. ESSAYS AND CASES.

“ V.—A Cursory Analysis of the Works of Galen, so far as they relate to Anatomy and Physiology. By J. Kidd, M. D., F.R.S., Regius Professor of Physic in the University of Oxford.

“ VI.—On the Treatment of Hypertrophy of the Heart, and Chronic or Sub-acute Inflammation of the Pericardium, especially in reference to the beneficial use of small doses of Mercury in those Affections. By Thomas Salter, Esq. F.R.S., Member of the Royal College of Surgeons, London; Fellow of the Royal Medical and Chirurgical Society, London: and Corresponding Member of the Hunterian Society, Poole, Dorsetshire.

“ VII.—Two Cases of Gangrene of the Lungs. By William England, M.D., Wisbeach, Fellow of the Royal Medical and Chirurgical Society of London; late Physician to the Norwich Guardians' Dispensary.

“ VIII.—A Case of partial Ectopia Cordis and Umbilical Hernia. By John O'Bryen, M.D., Bristol. (*Illustrated with Drawings.*)

“ IX.—Extirpation of the Eye, on account of a Tumour developed within the Optic Sheath. By R. Middlemore, Esq., Surgeon to the Birmingham Eye Infirmary. (*With Plates.*)

“ PART IV. REPORTS OF INFIRMARIES AND DISPENSARIES.

“ X.—A Report of the Out-Patients attended by F. Ryland, Esq. at the Birmingham Town Infirmary, between the 25th Dec., 1835, and 26th Dec., 1836.

“ XI.—A Report of the Out-Cases attended by the late George Parsons, Esq., at the Birmingham Infirmary, from January the 1st, to December the 31st, 1836. By Samuel Berry, Esq., Surgeon to the Town Infirmary.

“ XII.—A Report of Cases treated at the Birmingham Dispensary, from January 1st, 1837, to January 1st, 1838. By T. Ogier Ward, M.D., Physician to the Birmingham Dispensary.

“ XIII.—A Report of the Cases attended during the year 1837. By R. Middlemore, Esq., Surgeon to the Birmingham Eye Infirmary.

“ XIV.—Statistical Researches of the In-Patients of the Medical Wards in the Geneva Hospital, for the years 1834, 5, 6, to which are added some Documents relative to the Influence of the Seasons on the Development of certain Diseases amongst the poorer Classes of Geneva and its Environs. By H. C. Lombard, M. D., Physician to the Civil and Military Hospital, Geneva.

“ PART V.

“ XV.—Report upon the Influenza or Epidemic Catarrh of the Winter of 1836-7. By Robert J. N. Streeten, M.D.: with Observations upon the Meteorological Phenomena. By W. Addison, Esq. F. L. S.

Practical Observations on Hysteria, especially relating to its organic Character. By JOHN PRICHARD, M. R. C. S., &c. Leamington, 1838.

THIS is a production which it is difficult to describe. There is nothing put forward which bears any appearance of novelty, except the opinion of the author, that perforation of the stomach in females is often the result of an hysteric excitement proceeding from derangement of the uterus, but we must declare that the arguments of the author are utterly inconclusive. Of Mr. Prichard's medical notions we may form a judgment, when we find him stating, that whatever may be the form under which hysteria presents itself, he is convinced, “ that in each and every case it is to be regarded as emanating from uterine disturbance.” Shortly after, however, he divides hysteria into cerebral and neuralgic; the cerebral occurring after the functions of the uterus have ceased, the neuralgic while they are going on; and finally, he speaks of cerebral hysteria in *young females*, (page 32,) which is to be treated, after other means fail, by “ *a change to that state in which the maternal duties are assumed,*” and this “ *is to be regarded as certain to remove the evil.*” How much all this is opposed to practical experience, and how contrary is it to the knowledge (negative and limited though it be) that we possess of hysteria!

No one has ever denied that an hysteric female may become affected with organic disease, but every practical man knows that when such a change, consequent on local suffering, does occur, it forms an exception to the general rule.

Pamphlets of this sort, filled with loose reasoning, distorted facts, and unintelligible terms, such as our author's phrase of "*excited debility*," may answer their intended object, but can have no place among the real additions to medical science.

A System of Practical Surgery. By JOHN LIZARS, &c. &c.

WHEN we meet with a book written expressly for the use of surgical students, as a class book, we are not to expect to find in it any original views, or any important additions to our stock of surgical knowledge ; but we have a right to expect that the work shall at least come up to the present state of the science. And we are sorry to say that Mr. Lizars' work scarcely fulfils these expectations. As for example :

" With regard to fevers, I have already stated, that idiopathic inflammatory fever is caused by the application of cold to the body while heated. The same appears to be that of all other fevers, with the exception of hectic. I am of opinion, therefore, that neither putrid effluvia, marsh miasmata, nor pestilential vapours ever give rise to fevers."—p. 73.

Again, in speaking of the operation for the relief of empyema, he says,

" Let him (the surgeon) place the patient in the sitting position, with his back towards him, and measure both sides of the thorax ; then the affected should be wider than the sound side, the ribs more apart from each other, and remain nearly stationary during respiration, and on applying his ear, and shaking the patient, he ought to be sensible of a distinct undulation. He may then perform paracentesis thoracis, which is done with a bistoury and a gum elastic tube or catheter."—p. 153.

Now, this is the very case in which operation should be avoided, for the distinct undulation of which Mr. Lizars speaks, should inform him that there is air as well as fluid in the cavity of the pleura, and we know that air, in nine hundred and ninety-nine cases in the thousand, comes from a perforation of the lung itself, a circumstance which renders it almost impossible that the operation could succeed.

Speaking of emphysema from fractured rib, he says :

" The air inspired escapes by the wound of the lungs into the cavity of the pleura, accumulates, presses on the lungs of the affected side, and gradually condenses them, so as to overwhelm their functions ; the mediastinum is next pushed to the opposite side, so as to

reduce the expanse of the uninjured lung, and the circulation of the blood being thus impeded, death is inevitable."—p. 154.

This is not the mode in which traumatic emphysema proves fatal.

In treating of gun-shot wounds, page 19, we find Mr. Lizars countenancing the idea of the wind of a ball producing injury, and quoting Schmucker's case of a soldier whose queue was shot off: but if he will look into the fourth volume of the *Memoires de l'Academie de Chirurgie*, or into the Report of Observations made in the Military Hospitals of Belgium in 1816, he will find ample reason to change his opinion on the effect of wind contusions. Further, in speaking of the pathology of tetanus, says:

"This malady seems to consist in a lesion or excitement of the nerves, which rapidly involves the whole system. Dissection shows highly injected neurilematic sheaths of the roots of the spinal nerves, with the vessels of the membranes of the spinal cord loaded with blood; and these appearances according to the duration of the disease, as first pointed out by Dr. Saunders."—p. 198.

Would any student reading this, imagine that there was any question about the pathology of tetanus?

In speaking of tetanus also, p. 198, he states that the wound generally becomes unhealthy in appearance on the invasion of the disease. Now this is not generally the case: we have again and again seen not only the wound to retain its healthy appearance on the invasion of the disease, but to continue so during the whole progress of it.

From these few examples, chosen from many, we think we are borne out in stating, that this work does not fully represent the present state of surgical knowledge.

NUNNELY'S *Anatomical Tables*.

WE quite agree with the observation of the author of this little work, that anatomy cannot be properly learned from books alone, and we very much question the utility of multiplying works calculated to facilitate the system of learning anatomy by rote; or to speak more correctly, of acquiring the appearance of having learned, which the *Anatomical Tables* and all other works like it have a strong tendency to do; a "*multum in parvo*" is always the refuge of the idle, and may mislead the industrious for a time.

This work treats merely of the muscles, ligaments, fasciæ,

blood-vessels, and nerves. And these different departments of anatomical study are disposed of in a very summary, but as far as it goes, correct manner. Of the muscles and ligaments little is said, except to mention their points of attachment ; the action of the muscles is exhibited in a separate table, which appears to be rather carelessly done, as for instance, speaking of supination of the fore-arm, no allusion is made to the action of the biceps. Nor in the elevation of the humerus, no mention is made of the mechanism by which the trapezius bears so great a share in raising the bone, an action beautifully described by Winslow. The description of the fasciæ is concise and clear ; and it is much to be desired that the author's example in this would be followed by other anatomical writers on fasciæ. On the whole our author cannot be charged with many sins of commission. This little work has evidently been compiled with care and attention ; and we have no doubt, that while it may be useful to refresh the memory of one who once had mastered this branch of knowledge, it will, at the same time, deceive others into the belief that they have learned something of anatomy.

EVERS on Comparative Anatomy.

THIS manual, which has made its appearance within the last few days, carries with it the high recommendations of brevity and accuracy. It is just what the student has wanted, viz. a compendium setting forth not only an outline of the varieties of organization in animals, but at the same time furnishing accurate information on most of the essential details connected with their structure.

The book is well and neatly got up. Every student should possess himself of a copy.

SCIENTIFIC INTELLIGENCE.

On the blackening of Nitrate of Silver by Light, by M. Scanlan.—Nitrate of silver was recommended many years ago, as well as I remember, by Dr. John Davy, as a test of the presence of organic matter in distilled water. He showed that if nitrate of silver in solution be added to perfectly pure water, it is not altered by exposure to direct sunshine; but if the water contain a trace of organic matter, it will become blackened. It would seem to follow, as a matter of course, from this, that solid nitrate of silver is not blackened by exposure to light, (although the contrary is stated in almost every work on chemistry,) unless it be in contact with organic matter; and this we find to be the case by experiment. Mr. Ferguson, some years ago, when he had the management of the chemical laboratory belonging to the Dublin Apothecaries' Company, first showed me, that perfectly pure nitrate of silver is not blackened by long exposure to direct sun-light, but I believe he never gave further publicity to this fact than mentioning it to his chemical friends in Dublin, at the time. In consequence of some observations upon the blackening of this salt, made by Dr. Aldridge, of Dublin, in his review of Mr. Phillips's Translation of the London Pharmacopœia,* I was led to make the following experiment upon the subject: I took two cylinders of perfectly pure fused nitrate of silver, immediately after they were cast, from the mould, and wrapped one of them in paper, in the usual way that it is found in the shops; the other cylinder was transferred to a glass tube, and sealed hermetically, by means of the blow-pipe, without being suffered to come in contact with organic bodies; it was pushed from the mould into the tube by means of a glass rod; after a lapse of three days, the paper was removed from the first, and it was then sealed up in a tube, in a similar manner to the other. The two tubes were now exposed to the direct rays of the sun, and in half an hour

* Dublin Journal of Medical Science, March, 1837, and November, 1837. Mr. Scanlan mistakes. The Review, alluded to, was of the New London Pharmacopœia, not of Mr. Phillips's translation. The latter is an excellent hand-book for the student.

the nitrate of silver that had lain in contact with paper was blackened, while that in the other tube was not altered by six weeks constant exposure. The whole amount of blackening of the cylinder that had been papered was produced in the half hour. Nitrate of silver, free from organized matter, is sometimes blackened, I find, by exposure to the air; but this may be owing to the presence of sulphuretted hydrogen accidentally present; atmospheric air, too, is, I believe, seldom free from organic matter, at least in London, as may be seen, readily, by looking through a sun-beam.—*Report of the Eighth Meeting of the British Association. Athenæum.*

Abstract of a Paper on Ulcerations of the Cervix Uteri, and on the Abuse of the Speculum Uteri, by C. M. Gibert, M. D., Physician to the Hôpital de Lourcine. By Fleetwood Churchill, M.D.—This is an able paper, appearing at a very suitable time, when the assistance derived from the use of the speculum, in treating uterine diseases, is exaggerated far beyond the experience of practical men. The author commences by objecting to the practice of attributing the delicacy and disorders of females, leading an indolent life, to some derangement of the uterine function. “To place the seat of most of the disorders of females in the uterus, as some practitioners of our days have not hesitated to do, is to dress up an old error in the garb of improvement. But to treat actively, and by means more or less severe, all the changes in the cervix uteri, however slight and superficial they may be, is more than erroneous, it is criminal.

“This fault, however, has become so common, that, strictly speaking, the invention of the speculum (which led to it) must be regarded equally injurious as useful, if we consider the numerous abuses into which certain practitioners have been led by the constant use of the instrument.

“It is not denied that the diagnosis of venereal diseases has acquired much precision from the employment of the speculum, although much incertitude remains as to the real importance of certain lesions even when exposed to view.

“But I hesitate not to affirm, for my part, that certain alterations, altogether insignificant, and having no relation to the symptoms exhibited by the patient, have been many times regarded as the source of these symptoms. I have observed many women who complained of abdominal pains, whites, and other symptoms which might lead one to attribute them to derangements of the uterus, without there being any appreciable lesion of that organ. In other cases, certain changes were discovered by the aid of the speculum, which had given rise to no symptoms at all. This counter-proof places, beyond all doubt, the innocuous nature of many of the alterations revealed by the intervention of the speculum.”

After adducing a case illustrative of the errors of the “apostles of the speculum,” and commenting upon it, the author proceeds: “During the last two years that I have had the medical duty of the

‘Hopital de Lourcine,’ I have examined upwards of 800 women labouring under diseases of the genital organs, of which number 600 have been or are now under treatment in my wards. I am therefore in a condition to give testimony on the subject, interesting alike in the highest degree to science and humanity.”—“The neck of the uterus, examined by the speculum, differs in appearance in those who have not had children and with those who have been confined one or more times.”

“In the first case, the cervix is round, slightly pyramidal, and nipple-like, it is pierced in its centre by a small circular orifice. This is what is called the virgin cervix. In the second case, the neck is more or less changed in form and flattened; it is more voluminous; a transverse fissure, more or less gaping, with some irregularity of border, has succeeded to the primitive circular perforation. This is called the maternal cervix. It has rarely happened to me to find the uterus totally free from leucorrhœa,* and only in young females.

“The vagina is naturally of a pale rose colour and rugous; the neck of the uterus is white, slightly tinged with rose colour. Many circumstances of course may change both the form and colour of the cervix, so as to lead us to suppose disease when none exists. Even in the same person an examination of the speculum has given different results if made at intervals, shewing how utterly mistaken are those who attempt to decide upon the integrity of these organs after one examination.” After pointing out some errors which have crept into much practice, but to which we are as yet strangers in this country, M. Gibert proceeds, “But to return to the principal subject of this memoir, there is a lesion of the os uteri, which merits special attention, although not of the grave nature which some persons have supposed. This is a species of ulceration which I have termed ‘*Granular erosion of the cervix uteri*,’ (‘*erosion granulée du col de l’utérus*,’) and of which many examples are given in my manual of venereal diseases. This disease differs, however, from chancre properly so called. To give an idea of its frequency I may say, that out of 500 cases of disease, it was observed in 143. In some of them the erosion existed alone, without any other morbid symptom. With a few no leucorrhœa could be observed, and with others only a slight quantity of clear viscid mucus.

“With these exceptions (amounting to about fifteen) the patients affected with granular erosion presented more or less distinctly the other symptoms of syphilis.

“This granular erosion, which is rather superficial, is generally of a rounded form and circumscribed; it occupies sometimes the anterior, sometimes the posterior lip of the os uteri, occasionally both, and still more rarely it seems to penetrate the canal of the cervix. Its surface is granular and red, contrasting remarkably with the naturally

* In explanation of this remarkable fact, regard must be had to the class of females coming under the author’s examination.—*Trans.*

smooth surface of the neck. It bleeds when slightly touched. The surface of the erosion is ordinarily concealed by a layer of thick semi-transparent mucus, which it is somewhat difficult to remove.

“In the commencement this species of ulceration appears in the form of small granular points, slightly projecting, which soon excoriate and become confluent. It is very rare, however, for the ulceration to acquire any great extent.

“Though often connected with syphilitic symptoms it is impossible to attribute to them a venereal origin in all cases. Strictly speaking, the granular erosion is not a grave symptom. It neither gives rise to pain nor symptoms of metritis, as has been asserted. It may also be cured without having recourse to the *ordinary* energetic treatment. The cure, however, will generally take a considerable time. Cicatrization or stimulating applications do not appear to be of much use. More than once indeed the application of caustic has appeared injurious. The local remedy which has been most successful, is the spirituous tincture of nutgalls, or the “alcohol tannique,” as described by M. Boutegny of Paris.

“Mixed with eight parts of water, and used as a vaginal injection, it desiccates the erosion and arrests the discharge.

“Topical remedies, however, cannot supersede the necessity of more general treatment. In most cases, mercurial preparations have been advisable, and of these the proto-ioduret has been preferred, given to the amount of one grain in the day as a pill, and continued from six weeks to two or three months. Its exhibition should be suspended, however, if the patient be threatened with salivation.

“The womb must be preserved from all local irritation, but it is by no means necessary for the patient to be confined to the horizontal position.”

After this clear history of the disease in question, M. Gibert sums up the results of his observations in four propositions, with which we shall conclude.

1. It is inconvenient, useless, and dangerous (except in syphilis) to have recourse to the speculum uteri, unless there exist local symptoms indicative of organic lesion of the womb. Hysterical affections or abdominal pain are not to be deemed sufficient grounds for this investigation.

2. On the other hand, vaginal discharges, especially in females suffering from syphilis or suspected of it, demand an examination, by the ‘toucher’ and speculum, in order to a correct diagnosis. From this rule virgins or young girls are to be excepted.

3. Accidental redness of the neck of the uterus, aphthous patches and excoriations, slight ulceration, or granular erosion, are not of a very serious character, and never give rise to severe symptoms.

4. All these lesions may be cured without any local application. Yet in many cases the topical use of astringents (where the inflammation was neither too great nor too acute) has been decidedly useful. Caustics are rarely necessary, and when we have to employ them, the mildest and least painful should be chosen.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XVIII.—*Observations on the Treatment of Various Diseases.* By ROBERT JAMES GRAVES, M. D.

(Continued from Vol. XI. p. 408.)

HEPATIC ABSCESS OPENING INTO THE STOMACH BY THREE PERFORATIONS, ALSO INTO THE PERICARDIUM ;—PERICARDITIS ;—PLEURITIS.

THE following case contains many particulars of extreme interest, among which I beg to direct the reader's attention more especially to the physical phenomena produced by the simultaneous presence of air and fluid in the pericardial sac, no instance having been hitherto recorded where similar symptoms, arising from ulceration extended to that sac, have been observed.

In order not to lengthen the case too much, I have omitted the details of treatment ; they consisted of local depletion in the first instance by means of leeches, and an attempt to mer-

curialize the system, which attempt failed, because suppuration was in all probability established before it was made. My experience confirms the assertion made by Annesley and other writers on diseases of tropical climates, that it is impossible, or at least very difficult, to make the mouth sore to salivation, once the formation of abscess in the liver commences. Of course no practitioner, who is aware that hepatic suppuration has actually set in, will continue the exhibition of mercury ; it then becomes injurious. In the following case, when suppuration was ascertained, poultices were applied, and various astringents were subsequently employed, in vain, to check the diarrhœa.

Anne Walker, æt. 25, spinster, of spare habit and nervous temperament, on Thursday night, 13th inst. without any assignable cause, was seized with a sudden and violent pain in every part of the abdomen, extending to the loins and back, unprecedented and unaccompanied by any other complaint : was immediately bled, but without much relief ; continuing in the same state, venesection was repeated the next morning with more effect ; hot stupes were also applied. The entire of the 14th (yesterday) she remained in excruciating agony, applying the stupes, and obtained but little ease. She now lies on the back, with the legs drawn up towards the body, unable to turn to either side, or stir in the least in the bed, without an insupportable increase in her complaints : the pains she describes as of a lancinating nature, sometimes resembling the pricking of a number of pins, commencing at the epigastrium, shooting downwards to the pubes, and extending laterally into each hypochondriac and lumbar region.

Since the commencement of the attack she has been deprived of sleep ; much annoyed with constant thirst, and a nauseous, disagreeable taste in the mouth. Her countenance is now anxious and distressed ; skin moist, and covered with slight perspiration ; tongue white and moist ; pulse 128, small and somewhat wiry ; respiration 54 ; no morbid phenomenon can be detected in the chest ; heart's action rapid, and sounds na-

tural ; the abdomen is tense, hard, and exquisitely painful, the slightest degree of pressure causing much uneasiness ; bowels free ; urine passed in regular quantities.

17th. The greater part of the night was in a profuse perspiration ; the pains in the abdomen generally not so acute, they are, however, still aggravated by change of position ; the mouth has become tender, and gums spongy ; pulse 104, tolerably full, and easily compressed ; respiration 40 ; tongue coated, and moist.

18th. Since the poultices were applied, the pains have been so far lessened that she can extend her legs without their being increased ; her countenance is not so distressed, and she appears more at ease ; is at present in a profuse sweat ; pressure on the abdomen still occasions uneasiness. In the right hypochondrium and epigastrium there is a considerable tumefaction, somewhat of a conical shape, affording, when pressed, a degree of elasticity and *dulness on percussion* ; the pain produced in this part by pressure is very acute, whilst elsewhere it is comparatively slight.

19th. The only part of the abdomen pained by pressure is that where the tumefaction was observed yesterday ; it extends from below the ensiform cartilage to within a couple of inches of the umbilicus, also laterally occupying a space between three and four inches ; and to-day a sensation of fluctuation is communicated to the touch.

20th. A violent purging commenced yesterday, and continued the entire night ; stools numerous, eight or ten, liquid, and of a dark colour, each being attended with griping and kneading ; was much troubled with shiverings and pains in the back ; her breathing is more distressed, and accelerated, 44 in the minute ; pulse 132, small and hard ; tongue moist. No change has taken place in the appearances of the abdomen.

24th. There has been no return of the purging since the 21st ; the perspirations are diminished, and her general aspect is improved ; she now complains principally of pains in the

back, continued and shooting upwards along the entire of the spinal column. When the tumour is now percussed, *it emits a tympanitic resonance*; the lower part of the left side also is very clear on percussion; *cannot now detect the fluctuation observable on the 19th*; the elasticity remains as before; pulse 116, soft and improved in strength; respiration 30.

26th. Was troubled with a hiccough the entire night; had but little sleep, and sweated profusely; is quite free from pain except in back and loins; has no appetite, but great desire for drinks; the tumour appears flatter, is free from tenderness, and still tympanitic when percussed; pulse 128, small and soft, respiration 32; breathing regular. Tube of stomach pump to be passed into œsophagus.

28th. No air escaped after the tube was introduced; no change has taken place either in the size or sound of the tumour; bowels freed three times since yesterday, and stools attended with griping.

29th. The tumour in epigastrium is considerably diminished in size, percussion elicits, as before, a tympanitic resonance, but does not extend, as on previous days, to the right hypochondrium; her countenance is improved, and spirits not so depressed; breathing continues too free, and pulse rapid.

Oct. 1st. Purging has returned, with griping pains in the abdomen, and numerous liquid stools; the tumour in abdomen is scarcely perceptible, and but a slight degree of clearness on percussion can be elicited; the upper part of the tongue is extremely painful; on the dorsum there are two or three sores, the largest about the size of a silver penny; the others resemble fissures, and are separated from each other by septa: pulse 116, soft and tolerably full; respiration 32.

2nd. Purging remains unchecked; the tumour in abdomen has altogether disappeared; no tympanitic resonance is now afforded by percussion; the sides of the tongue this morning are covered with aphthæ; the sores on the dorsum remain the same.

3rd. No effect has been produced on the purging, was up-

wards of six times to stool since yesterday ; is much reduced in strength ; countenance pale ; pulse quick, 112 ; has great thirst ; tongue dry, and not so sore.

6th. Heart's sounds natural. Percussion and respiration over both lungs as in the healthy state : abdomen sunken and free from pain.

7th. Bowels have been opened seven times within the last twelve hours. Pulse 120. Respiration 30.

9th. *Was attacked yesterday with acute pain in the cardiac region, and last night had a violent beating of the heart, also a burning heat below the left breast.* She cannot recollect any cause to which she might attribute this. Her present state is extreme emaciation and debility, cheeks hollow, eyes sunken, countenance dejected, and spirits languid ; her breathing remains accelerated, short, and distressed ; the jugular veins in the recumbent posture turgid, but without pulsation ; likewise those along the trachea.

Percussion over chest generally is clear, except at the inferior and middle portions of the left side. Respiration in these parts is feeble, elsewhere pure and loud : impulse of heart perceptible, but feeble. About half an inch distant from the lower edge of the mamma both sounds are confused, and a slight bruit de soufflet is audible ; advancing to the right it increases in roughness, and below the mamma it becomes a complete creaking noise, accompanying both sounds of the heart, and is still louder between the sternum and breast ; when pressure is applied it gradually increases these phenomena, and when considerable pressure is used, they are changed into a loud frottement, obscuring both sounds, the first especially ; they are also rendered more distinct by holding the breath.

Abdomen smaller ; purging stopped ; pulse 130, small and compressible.

10th. The phenomena are now audible as far as the middle of the sternum, over the cardiac region, and laterally, being in each place of the same character. The sound is between bruit

de soufflet and bruit de scie, in a great measure masking the first sound and accompanying the second, which still retains its clearness. Immediately under the mamma, together with these sounds, but heard only occasionally, is a *peculiar metallic click*, affording the idea of some fluid dropping in or about the pericardium ; it is removed when pressure is made over the heart, whilst the other noises undergo a thorough change ; thirst urgent.

11th. Has not had a return of the pains in the left side ; sweats every night as much as hitherto : had several shiverings last night, after each of which she fell into a copious perspiration. Pulse 136, feeble ; respiration 40 ; bowels regular.

Impulse of heart is feebler ; when the hand is placed over it a rubbing sensation is communicated.

The sound to-day has assumed the character of an emphysematous crackling, is very fine, and obscures both sounds of the heart ; is more distinct along the middle and inferior parts of the sternum, and can also be heard to the left of the mamma. The metallic click, or apparently the dropping of fluid, observed yesterday, is more audible and distinct, but irregular in frequency.

12th. The irregular click, audible yesterday only at intervals, has now become a *loud metallic ticking, audible at each stroke of the heart* over those parts where the emphysematous crackling and other sounds were to be heard ; it obscures all the phenomena hitherto noted, except a slight bruit de soufflet about the nipple of the left mamma. Impulse cannot be felt. Is sinking fast.

13th. Died last night at 10 o'clock.

Autopsy Twelve Hours after Death.—Percussion over the front of chest afforded no evident dulness ; over the cardiac region it was clear. When the sternum was raised, both lungs were found collapsed ; the left in particular, which was found compressed by a quart of sero-purulent fluid. Weak adhesions connected both lungs with the external pericardium ; and their inferior lobes with the upper surface of the

diaphragm. The pericardium appeared enlarged, and a small quantity of fluid could be felt.

The abdominal parietes being removed, the cavity of a large abscess was exposed, situated in the left lobe of the liver. Its form was circular, about eight inches in circumference, and bounded anteriorly by a portion of the parietes of the abdomen, and ensiform cartilage. Its posterior wall was formed by the remaining solid part of the left lobe; whilst the diaphragm superiorly was in immediate connexion with it, and the falciform ligament served as a means of separation between it and the right lobe: its thin edge was over-lapped by a portion of the stomach; and near the pyloric orifice was an ulcerated circular hole, with rounded and smooth edges, about three-quarters of an inch in diameter, communicating directly with the abscess. The stomach was intimately connected with the sub-surface of the left lobe by its concave margin; and near to its cardiac extremity were two other openings, one somewhat oval in shape, about half an inch in diameter, and connected with the abscess by means of a canal capable of admitting the tip of the little finger, and separated from the other by a thick band, evidently a portion of the stomach. This last perforation, or the one nearest the œsophageal extremity of the stomach, had no communication with the abscess. The surface of the abscess is irregular, presenting many depressions and elevations; its colour of a yellowish grey, its substance creamy, soft, and reduced by pressure into a pus-like fluid; when cut into, it is at least three quarters of an inch in depth, but does not retain the same thickness in every part; beneath, the structure of the liver is visible, and in firm connexion with it the stratum of diseased substance, neither can it be separated from it.

Where the diaphragm and pericardium are united, is a perforation sufficiently large to admit the middle or ring finger, and opening directly from the abscess into the pericardium; the edges are ulcerated and uneven; and within the covering of the heart are about two ounces of yellow-coloured fluid

mixed with flakes of lymph. The pericardial sac is increased to four times its natural thickness, but appears equally dense in all parts ; its external surface is highly vascular ; its interior is likewise inflamed, dotted with numerous red spots, in some parts about the size of a pin's head, and in others forming an arborescent appearance ; the surface has in a great measure lost its natural glistening appearance, and looks uneven, being coated in parts with small portions of organized lymph ; and generally, particularly towards the origins of the great vessels, with small, granular, semi-transparent bodies resembling millet seeds, or the eruption sometimes seen in cases of rheumatic fever : its feel is quite gritty, but when these bodies are scraped off, the serous lining of the pericardia is apparent underneath.

The heart itself is of a light red colour, and its investing membrane is covered, like the pericardial sac, with those granular substances, more abundant about the auricles and base of the heart. Both auricles are bound down to the substance of the heart, by means of strong, tough, and organized pieces of lymph.

Some tubercles scattered through the superior lobe of each lung. No adhesions existed between the peritoneum and intestines, or between these latter.

I am indebted to my talented and indefatigable clinical clerk, Mr. Thomas Moore, for the preceding report of the progress of this singular case, concerning which the following remarks appear necessary :

1st. When the abscess burst into the stomach, the epigastric tumour which the abscess formed, did not at once subside, but suddenly, from having yielded a dull sound on percussion, became tympanitic and clear ; air from the stomach having found its way into the cavity, while the pus escaped.

2ndly. The now tympanitic tumour seemed so exactly to resemble the stomach distended with air, that we were induced to pass a tube into the stomach, but it did not give vent to any air.

3rdly. In a few days the air also passed from the cavity of

the sac, then all traces of the tumour entirely and unaccountably disappeared.

4thly. The diarrhœa was caused by the perpetual flow of fetid and irritating matter from the abscess into the intestinal cavity.

5thly. No peculiar symptom, pain, or derangement of its functions, denoted the extensive ulceration of the stomach.

I shall revert to this subject after the details of the two following cases of ulceration of the stomach have been laid before the reader.

6thly. The inflammation spread by continuity of structure, from the abscess to the pleura and pericardium *in the first instance*.

7thly. Soon after the pericarditis thus formed had commenced, and at the time that its usual physical phenomena were clearly perceived, a new set of physical phenomena arose, *dating from the moment the pericardium was perforated, and air entered its sac*.

8thly. Although most intense general peritonitis existed when the patient was admitted, yet no traces of general peritoneal inflammation were discovered on dissection.

9thly. It may be asked, why I had not recourse to an operation to let out the matter, as soon as fluctuation had become plainly perceptible in the hepatic tumour? My answer is, that the tumour formed so quickly, and seemed to tend to the surface so rapidly, that I thought it better to wait for a day or two, in order to render the operation safer, never anticipating that the matter could, in so short a time, find an exit by another channel.

ABDOMINAL ABSCESS OPENING EXTERNALLY, AND COMMUNICATING
ALSO WITH THE STOMACH.

Catherine Delany, aged 56, a washerwoman, was admitted into the Meath Hospital, on the 5th of May, 1833; she had

a very large abdominal tumour, which made its appearance about two years previously, and was first perceived in the left hypochondriac region. It slowly but gradually increased in size, and did not appear to affect her health, for she was able to work until a few days before her admission. The tumour was globular, felt uneven and rather solid, and was well defined in its outline; occupying the whole of the umbilical, extending upwards into the inferior portion of the epigastric, and downwards into the superior portion of the pubic region. Laterally it stretched considerably into the right and left lumbar regions. It was quite moveable, and always fell towards the side on which she lay. It had lately, *and but lately*, become painful and tender, particularly about the navel.

The length of time this tumour had been growing, its shape, and the absence of all constitutional affection, or local pain, during so long a period, induced me to consider it as ovarian. Shortly after her admission, matters began to wear a more threatening aspect; the tenderness and pain felt in the tumour increased daily, and she now was troubled with frequent returns of nausea, which in the course of a fortnight was succeeded by obstinate vomiting.

The tumour began to grow red and softer in the umbilical region, where a deep seated fluctuation was recognizable, which soon became quite evident and superficial, accompanied by heat and deep redness of the integuments, and a surrounding hard margin. In fact, every thing announced a collection of matter rapidly making its way to the surface. In consultation it was determined not to open this, for several reasons, the principal of which was, the long continuance of the local disease seemed to preclude all hopes of ultimate recovery: in the mean time the pain, emaciation, and suffering of the poor patient increased, and while the central softening of the tumour rapidly progressed, its circumscribed and solid structure towards the circumference as rapidly subsided, so that although the bulk of the whole was probably the same, its shape and prominent appearance were

much altered. The vomiting became more distressing, nothing was retained in the stomach, large quantities of fluid deeply tinged with bile were thrown up for a week or ten days; about the 8th or 9th of June, the fluid ejected suddenly changed its character, being now a thick, viscid, and glairy mucus. On the 13th the tumour burst, and continued to discharge daily nearly a gallon of fluid exactly similar to what she had lately vomited. The external opening evidently communicated with the stomach, for the moment any fluid was swallowed a portion of it was forced out through the former. On one occasion a piece of orange, which she had chewed and swallowed, blocked up the external orifice for several hours. It is well worthy of notice, that notwithstanding the deplorable ravages committed on her organs of digestion, and notwithstanding the existence of a perforation in her stomach, the tongue continued, throughout the whole of her illness, clean and moist! Again, when the perforation had taken place, the vomiting ceased and although her most urgent sensation was that of thirst, yet she had a tolerably good appetite, which she sought continually to gratify by swallowing jelly, &c.! She lived four days after the tumour burst externally, and about nine days after the occurrence of the perforation of the stomach. The external orifice communicated with a very large sac, the seat of the abscess, and formerly, in all probability, the sac of the tumour before it had begun to suppurate. This sac extended over the whole space formerly described as occupied by the tumour, and contained a considerable quantity of thick gruel-like fluid. No solid matter whatsoever was found within the limits of the tumour; nothing remained but the sac, thickened by inflammation, and adhering by pseudo-membranes to all the neighbouring viscera. The intestines and great omentum, matted together, formed the posterior wall of the sac, but on account of the diseased state of the parts it was impossible to determine with certainty, whether the anterior wall was formed by the peritoneum lining the abdominal muscles, or by the sheath of the recti. The former supposition seems the most probable, for a large portion of the surface of the liver was

within the cavity of the abscess, and at its inferior edge, was destroyed by ulceration. The opening into the stomach was in its greater curvature, and was distant from the pylorus about an inch and a half, and with the loss of substance in the liver was the result of simple ulceration, without preceding scirrhus. All the intestines and viscera behind the tumour were, without exception, free from disease. I cannot conjecture in what structure this disease originated, or what was its nature at the commencement, but it may be doubted whether an operation for letting out the matter might not have prolonged, if not saved the patient's life, had it been undertaken at the time fluctuation first became perceptible, and before the ulceration of the stomach and liver had commenced. The details I have given may possibly serve as a guide to others, should another such case occur.

CASE OF CHRONIC INFLAMMATION AND ULCERATION OF THE
MUCOUS MEMBRANE OF THE STOMACH.

Last summer I was requested by my friend Doctor Henry, to meet him in consultation on the case of a gentleman residing in Gardiner-street. Our patient was about 50 years of age, and had previously enjoyed good health. We could not ascertain the cause which had given rise to the disease, which lasted about two months, terminating fatally. The symptoms underwent very little variation, and were accompanied by an extreme pallor of the skin. He had, indeed, very much the wax-like aspect of a person exhausted by repeated hemorrhage. He sunk very gradually, having fallen into a state of extreme emaciation.

Some of the principal symptoms are described in the following note which I have received from Doctor Henry :

“ Fitzwilliam-square, Oct. 22nd, 1838.

“ MY DEAR SIR,

“ I find on referring to my notes that I have not much to add to what you already know of the case in Gardiner-street. You saw, yourself, the progressive emaciation

and debility ; the total loss of appetite ; the insatiable thirst ; a thirst greater and more insatiable than I ever before witnessed, lasting, as it did, during the whole course of my attendance. The eagerness with which the patient looked at any drink which was pouring out for him, and the impatience with which he seized the vessel and swallowed its contents at one draught, was the first circumstance which determined my diagnosis of an inflammatory process going on in the stomach.

“ In this case there was, *besides, a total absence of pain on taking food or drink, or from pressure on the region of the stomach, also an absence of vomiting*, except when it happened that the patient had taken a large quantity of any warm liquid. He was then (and only then) sure to vomit ; but he never threw up any of the solid food which he used to take in small quantities from time to time.

“ The patient occasionally hawked up a spit in which there was contained a small globule of rose-coloured blood of the size of a pea ; this globule of blood was intangled in the mucus, but without discolouring or streaking it.

“ The patient died from inanition.

“ All the internal organs were healthy, except the stomach. The interior of the stomach presented a dark surface when opened. The portion surrounding the cardia, and the greater part of the large extremity, were almost quite black, but without any appearance of large veins. The blackness was uniform, and seemed as if the substance of the lining membrane was deeply and permanently dyed with Indian ink. Around this black part was a circle of florid-red, gradually merging in the parts beyond, which were of the dark colour of ordinary melæna, with large black veins. Near the pylorus were two or three florid-red patches, evidently of superficial ulceration, with a defined hard red border. They were of the size of a shilling or split bean. The pylorus itself was healthy. The patient derived most relief from repeated small draughts of iced water. No

medicine was of the least service. It seems strange that in this state of the stomach sulphate of iron did not disagree.

“ I am, dear Sir, truly yours,

“ JAMES HENRY.”

In addition to these symptoms it should be observed, that the patient's tongue was constantly parched. He slept, however, remarkably well during the greater portion of his illness, and the evacuations from the bowels were throughout *perfectly natural*. His belly did not exhibit at any time the least tumefaction, or the epigastrium any tympanitic distention ; his pulse was in general about 94, and was not hard or wiry. Failure of strength and loss of flesh were amongst the earliest symptoms, and progressed steadily in a ratio beyond all proportion greater than could be expected, considering the quantity of nutriment taken and the well digested appearance of the fæces. The urine was quite natural, except in the beginning, when it was for a time tinged with blood.

In comparing the three preceding cases together, the attentive reader will not have failed to remark how few were the symptoms denoting any injury of the stomach in the two first, where the ulceration of that organ was, nevertheless, most complete and extensive, it would seem, indeed, as if the perforations, as resulting from matter making its way through that organ, were accompanied by much less derangement of its functions, than a far less widely extended inflammation originating in the stomach itself spontaneously. The perforating process, intended to accomplish the evacuation of an abscess, must therefore be regarded as a curative effort of nature, wisely and beautifully so contrived that the steps necessary to insure the escape of the pus may be accomplished without endangering life or compromising the health of the stomach. If this be so, and it is scarcely possible to doubt it, we are presented with an additional example of the futility of *a priori* reasoning, for surely no one who examined the extensive perforations in the stomach of the

two first cases, would have hesitated to pronounce that lesions of tissue so profound and extensive must have produced corresponding injury of function.

In the third case it is well worthy of notice that many of the symptoms reputed to be of most constant occurrence in gastritis, were absent. There was *no tenderness, no vomiting, no pain on taking food, and no epigastric distention.* Neither did this long continued and at length fatal gastritis, ever give rise to the least mental aberration, or disturb the soundness of sleep. How many reasonings and explanations of the *gastritic* origin of typhus fall to the ground before such a case!

ON THE COMPARATIVE PREVALENCE OF FEVER DURING THE LAST
TWELVE MONTHS.

Having remarked an almost total absence of fever cases in the wards of the Meath Hospital, during the month of September 1838, I was led to make inquiries relative to admissions into other Institutions, and accordingly applied to Doctor Gordon Jackson, one of the Physicians of Cork-street Fever Hospital, who obligingly obtained for me the following return:

*Number of Patients admitted into Cork-street Fever Hospital,
for each of the last twelve Months.*

DATE.	MALES.	FEMALES.	TOTAL.
1837.			
October, . . .	152	217	369
November, . .	136	234	370
December, . .	167	261	428
1838.			
January, . . .	170	335	505
February, . .	177	319	496
March, . . .	158	352	510
April, . . .	119	174	293
May, . . .	101	169	270
June, . . .	93	127	220
July, . . .	66	136	202
August, . . .	79	145	224
September, . .	87	158	245

This list suggests some very important reflections. In the first place, it affords very strong evidence of the incorrectness of the commonly received opinion, that windy and rainy weather are less favourable to the spread of fever in Ireland, than calm, dry weather, for the latter qualities have been remarkable in the month of September, which was unusually fine and entirely without rain. During January, when fever was so rife, no rain fell, for the weather in the commencement of the month was dry and fine, and afterwards an intense, dry cold, and frost prevailed, so that, as to the absence of rain, January and September quite agreed. Again, the popular opinion that hot weather tends to favour the spread of typhus, is contradicted by our data, as is evident from comparing the four warmest months of the year, May, June, July, and August, with December, January, February, and March.

Much stress has been laid on dearth, bad harvests, and unproductive seasons as tending to favour the propagation of typhus in Ireland. Thus, in the great epidemic which ravaged the west of Ireland in 1822, I have elsewhere proved that the most distressed and poorest districts were also the most infected with fever. The period I have chosen embraces twelve months, during which the inhabitants of Ireland subsisted on one and the same harvest, on the whole a very good one, and yet we find an immense difference as to the prevalence of fever in Dublin at different periods of the year. Being anxious to ascertain whether a similar disproportion was observed in Cork, I wrote to Dr. Edward Townsend, who forwarded to me the following return :

*Monthly Report of Fever Patients admitted into the Cork
Fever Hospital from 1st Oct. 1837, to 30th Sept. 1838.*

1837.

October,	210
November,	172
December,	165
	<hr/>
	547

<i>Brought forward,</i>	. 547
1838.	
January,	165
February,	160
March,	164
April,	142
May,	176
June	136
July,	105
August,	107
September,	84
Total, . . .	1786

Doctor Townsend also remarks in his letter to me, that fever had been unusually rare in private practice, during the latter months of the above period; and the same observation applies to Limerick and Belfast, for which towns the public or hospital returns are as follow:

*Admission of Fever Cases into the Limerick Fever Hospital,
in 1837-8.*

October, 1837,	189
November,	191
December, 1838,	173
January,	127
February,	118
March,	127
April,	159
May,	165
June,	158
July,	160
August,	152
September,	121
Total, . . .	1840

The return from Limerick was kindly communicated to me by Dr. William Griffin; that from Belfast by Doctor M'Cormac.

The mortality is annexed to the Belfast Report. From this it appears that the fatality diminished in direct proportion to the frequency of the disease. This is contrary to the usual result of experience, for in general where fever is not epidemic, the cases which do occur are in proportion more fatal. This was exemplified in Limerick during the last year, for Dr. Griffin joins Dr. Geary in attesting, that they never remembered a year in which so few cases of fever occurred in private practice; they were, however, of a very bad type, and unusually fatal. Perhaps, after all, the discrepancy between the Belfast returns and general experience may be more apparent than real, for it is very possible that during the epidemic, none but decided cases of typhus were admitted into hospital; but when fever became much less frequent, cases of a comparatively mild nature might have been admitted, as no press of a worse form of disease existed to authorize their exclusion.

DATE.	ADMITTED.	DISCHARGED.	DIED.
1837.			
October,	194	219	29
November, . . .	233	212	32
December, . . .	183	174	26
1838.			
January,	173	151	23
February, . . .	127	100	18
March,	107	121	16
April,	69	87	6
May,	99	72	17
June,	79	81	8
July,	81	63	8
August,	89	104	8
September, . . .	76	52	8
	1510	1436	199
Remains 1st Oct. 37	196	199	
	1706	1635	
Remaining in Hospital, 1st October, 1838, 71.			

On comparing together the preceding tables, the coincidence is remarkable, and proves that the chief causes of the epidemic diffusion of fever in Ireland must be of a very general and not of a local nature, for we here find the most exact agreement between results observed in cities far asunder, and widely differing in aspect and position. It must have been an influence coextensive with the island, and acting every where with a nearly equal degree of intensity, which brought about this coincidence, and made fever attain its maximum and minimum at the very same time in each of these four cities. It is well to keep in mind that the establishment of the existence of this epidemic influence, (which in Ireland, even when at its minimum, is but too productive,) does not preclude us from admitting, that many other causes of minor importance may in Ireland give rise to typhus; among these we may reckon catching cold, fatigue, mental emotions, and contagion.

October 25th. Since the above notes were written, fever has again become more frequent, and the cases admitted have been unusually severe, the maculæ profuse and dark, and in some instances mixed with a bluish bruised-like appearance of large spots of the skin; in fact the disease would almost justify the title of putrid fever. This change for the worse in frequency and in type, has been accompanied by a change in the weather, which having been seven or eight weeks dry and calm, became about the 10th of October stormy, uncertain, and frequently rainy, which is exactly the weather whose reputation has been hitherto the best!

The type of fever now prevalent commenced about this time three years in Dublin; and as it had been very destructive, I was in hopes, when it had nearly disappeared in autumn, that its malignant character would have been extinguished, and we might look for a milder variety. In this, however, we have been disappointed, and for another winter, at least, are we doomed to contend with our old antagonist, still in full possession of its former virulence.

ENLARGED AMYGDALÆ.

When common cynanche tonsillaris, scarlatina, measles, or any other disease which induces inflammation of the throat, attacks persons of a scrofulous habit, enlargement of the amygdalæ is a very frequent consequence. In children it is more common than in adults, and when it takes place it requires prompt attention, for if these glands be permitted to become hypertrophied, and to remain so for many years, their size becomes at last considerable, and they may be perceived as large as walnuts, leaving but slight interval between them, so that the disease being confirmed, the patient, when he grows up, is constantly annoyed by an irritation, which, in many, produces a slight hem or occasional hawking, and in all is the source of much inconvenience or even danger, when the person, from cold or any other cause, is attacked with sore throat. Then the inflammation which, under other circumstances, would be moderate, assumes a great degree of violence, the amygdalæ swell suddenly to an excessive size, and the attack is both severe and long continued.

These facts prove the propriety of endeavouring to restrain enlargement of the tonsils in children. After acute diseases, time, with a tonic regimen, country air, tepid salt-water baths, and sea bathing, will frequently remove this affection, particularly if assisted by gargles, such as warm salt-water, a solution of sulphate of zinc, or infusions of astringent vegetable substances with alum, &c. &c. When these means fail, we may try the daily application of tincture of iodine, mixed with a little treacle.

The principal remedy, however, is the nitrate of silver; many use this in solution, but I prefer Mr. Cusack's method, which is as follows: The solid stick of lunar caustic, or some of the latter in powder, and placed in a proper instrument, must be kept steadily pressed against a particular spot of the enlarged gland; two three, or five seconds will suffice to

secure the formation of a small eschar, which falling out, will leave in the part, when healed, a slight depression like the largest pit formed by a small-pox pustule. When this has been effected, which is usually in about five days, a similar proceeding must take place with the other amygdala; and so on with each, turn about, until the desired reduction of size has been accomplished. When the glands are large, this process usually requires about six months; it is slow but sure; and must be intermitted when any accident gives rise to temporary sore throat or to catarrh.

Some use ligatures to reduce these glands in size, and others cut them out; the latter operation is not altogether free from danger, as was proved in the case of a patient of mine, who, contrary to my advice, went to Paris to have it performed. The left amygdala was excised, and the gentleman was very near dying of the consequent bleeding.

SINAPISMS.

This species of rubefacient is applied in various diseases, viz. in the latter stages of fever, in pleurodynia, colic, pains of the stomach, and not unfrequently in suppressed or irregular gout, where it is attempted to fix the disease in the extremities. Nothing is more certain than that gout may go astray, and that it may, occasionally, be called away from important internal parts, by means calculated to excite inflammation on the surface. If a man, in whom a fit of the gout was about to take place, sprains his ankle, inflammation of the part is forthwith the consequence, and there the gout at once settles. Within a short period of time I have seen three remarkable examples of the relief which vital organs may experience when gout appears in the extremities. A publican applied to me with violent pain in his stomach, which came on every evening, and lasted many hours in spite of every remedy. In a day or two he got a violent attack of podagra, and had no more internal suffering. A gentleman whom I attended with Mr. Barker, was attacked

with cerebral symptoms and indistinctness of vision and utterance. We feared hemiplegia; the next day he got severe podagra, and was able to speak perfectly well, and see distinctly. He was 75 years of age. At the very same time I was attending, with Mr. Colles and Mr. Haffield, a robust and powerfully made gentleman, aged 74, who having had symptoms of flying gout, and shortly after a bowel complaint, made use of the salt-water plunge bath. This imprudent act brought on a violent and nearly fatal hemoptysis. He was bled twice, and got the usual styptics with relief, but his improvement became much more rapid when gout appeared in both his feet. Facts such as these occur frequently, and leave a strong impression on the mind of the practitioner of the prudence of attempting to bring the gout to the extremities in similar cases. Some try to do this by means of stuping, liniments, blisters, or sinapisms; but it appears to me that the latter are seldom applied in a manner likely to effect the desired object, for when composed of the usual ingredients, sinapisms act too quickly to be long borne, and of course, only give rise to a very superficial inflammation and that of very brief duration. To fix gout in a part, e. g. in the foot, our application must act much more gradually, and must excite the deeper seated tissues. These objects may be obtained by mixing one part of strong and fresh ground mustard powder with three of flour, and adding as much treacle as will convert them into a viscid paste, which may be spread like a plaster on linen, and applied to the part. This will be borne for four or six hours, and will cause a redness which will last a whole day. The proportion of flour may vary according to circumstances.

NEURALGIA OF THE TESTICLE.

This is not a very common form of disease, but it requires notice, as it gives rise to excruciating agony, and constitutes one of the most painful affections that can be imagined. I have seen two examples of it within the last year; the first was a

young gentleman of highly irritable nerves, who had studied hard and dissipated much ; in him the paroxysms of pain did not observe any very marked period, but returned daily at uncertain intervals, which grew shorter and shorter, until at last he had scarcely any respite day or night. There was no fever, and not the slightest appearance of local congestion or inflammation. When attacked with a paroxysm the patient would throw himself on the floor, and roll about in the greatest agony, covered with a cold perspiration. This case yielded to large doses of carbonate of iron freshly prepared, and frequent inunction of the testicle and cord with belladonna ointment. The second case of neuralgia of the testicle occurred in a gentleman who laboured under neuralgic pains, decidedly of a gouty nature. In him the pain of the cord and testicles used to come on every afternoon about four o'clock, and continued for several hours. The pain, though considerable, did not approach the degree of agony experienced in the first case. It was at times, however, so severe as to compel him to groan aloud. This neuralgia of the testicle disappeared after a few days, and was replaced by a violent gouty pain in the loins and right hypochondrium. The latter yielded to the usual local treatment and the use of colchicum internally.

NEURALGIA OF THE LARYNX.

This occurred in a young lady originally of vigorous constitution, but latterly suffering from menstrual irregularity and hysteria. The laryngeal affection had been considered to be inflammatory in the country, and had been treated with purgatives, leeches, blisters, antimonials, and finally mercurialization. No relief had been obtained, and she came to Dublin where she was placed under my care, and that of Doctor Marsh and Mr. Barker. The pain had become almost constant when we first saw her, but was by no means violent, except now and then when it used to become suddenly aggravated. These paroxysms of pain could not, even properly speaking, be called

violent ; they were however, distressing, amounted to a most annoying feeling of distress about the whole region of the larynx. There was no external tenderness, and the internal fauces were healthy. We considered it to be an hysterical nervous affection. This neuralgia was chiefly remarkable for a change of tone and weakness in the voice which invariably attended the paroxysms, shewing that the *rima glottidis* and the *chordæ vocales* were the parts chiefly implicated. We must suppose, therefore, that the pain was derived from the branches of the superior laryngeal nerve, which Dr. Reid has proved to be chiefly *sensitive*.

The alteration of voice which accompanied the paroxysms of pain must be considered as a proof that the superior laryngeal nerve has some influence on the motions of the vocal organ, unless indeed, we adopt the supposition that the affection extended likewise to the inferior laryngeal nerve. The facts of the case contain nothing decisively confirming or negating either hypothesis.

We first gave large doses of carbonate of iron, which had the effect of rendering the attacks periodic. Every morning, at *ten o'clock to the minute*, the paroxysm commenced. The dose of iron was now increased, afterwards sulphate of quinine, and finally arsenic were employed, but without any corresponding improvement. The degree of suffering became, indeed, less severe, and its duration less protracted, but it appeared extremely doubtful whether the improvement was not owing more to time than to medicine. Under these circumstances we thought it prudent to desist from all active treatment, and we recommended change of air, scenery, and the use of chalybeate mineral waters. I believe she still continues to suffer, but in a less degree. A year has now elapsed since I last saw her. This case affords a striking example of the curious fact, that medicines administered for the purpose of relieving a disease more or less fluctuating or remittent in its character, will sometimes render it strictly periodic, with marked paroxysms and free in-

tervals. Having produced so striking an effect with our remedies, we are apt to calculate with confidence on still further improvement, and we increase the doses of tonics with boldness and full of hope ; disappointment, however, here awaits us, for no tonic will be found capable of effecting any further alteration or shortening of the fit. In such cases we cannot be too much on our guard, lest we injure the constitution by too frequent attempts to procure a diminution of suffering.

INFLAMMATION OF THE SPINAL MARROW.

This disease is closely connected with the subject of neuralgia. *Myelitis* is so liable to be confounded with a great variety of painful affections, that every ascertained case of inflammation of the spinal marrow ought to be recorded for the purpose of rendering more perfect a department of pathology already diligently, but not completely cultivated. A young married woman, named ———, was admitted into the Meath Hospital, on the 12th September, 1838. She was healthy until the period of marriage, soon after which her husband commenced a system of ill-usage comprising beating, kicking, throwing down stairs, &c. &c. He was frequently drunk, and occasioned her every species of grief. No wonder that a life like this should have reduced our patient to the truly miserable condition she was in. She had been injured so often that it was difficult to say to what particular act of violence her present malady ought to be referred. She is much emaciated, respirations hurried, and pulse very quick. Has no headach, but complains much of agonizing pains in the loins, aggravated by pressure of the lumbar spinous processes, extending round the abdomen, and downwards to the hips and thighs. There is no pectoral affection, and her tongue, state of stomach, and general appearance, are not those of a person labouring under fever. She writhes in the bed from the violence of the pains ; she does not sleep night or day, and disturbs the other patients by her cries.

Blood was drawn by cupping from the loins ; leeches were

applied, and Dover's powder administered. Her extreme emaciation prevented us from adopting either more active depletion by the lancet, or the use of calomel. In short, we sought to relieve not to cure, for her death appeared inevitable. Blisters we could not apply, on account of the great emaciation. On the 15th we found that she had been screeching all night, and constantly wanting extract of opium, which was ordered her as a palliative. On the 16th she complained that the sense of feeling was leaving her thighs, and she died on the 18th, five days after admission. *On dissection* we found all the viscera healthy ; there was extreme atrophy of the intestines, especially the colon and cæcum, so it is probable that starvation was among her afflictions. The lower portion of the spinal marrow and the *cauda equina* exhibited an excessive vascularity and redness, but no exudation of lymph. Each nervous fasciculus of the cauda exhibited a vein on its posterior surface distended with blood ; and the remaining portion of each fasciculus displayed great arterial vascularity. For this and the following case I am indebted to Mr. Brady, a practising pupil of great promise.

HÆMORRHAGIC PLEURISY.

James Maher, æt. 22, admitted Sept. 4th, 1838, in a low emaciated condition. Has a very troublesome cough, which occurs in paroxysms ; sputa scanty and bronchitic ; can lie easier on his back than on either side ; sweats after sleeping ; appetite bad ; bowels open ; pulse 100, small ; respirations hurried.

Physical Signs.—On looking at his naked chest, it is evident that the right half of the chest moves much less than the left. Percussion yields a dull sound at the lateral and posterior regions of right side, in which latter region there is bronchial respiration without any rale ; in the former there is an absence of respiratory murmur ; there is a bronchophony approaching to ægophony posteriorly ; whereas, laterally the voice is heard

much less than in the natural state ; the intercostal spaces are not distended ; the left side is normal.

History.—States, that about the middle of August he fell in a fit upon his *left* side, and was bled four or five times largely for the apoplectic symptoms. In three days after he got a severe stitch in his *right* side, for which he was twice copiously bled and blistered, and took some calomel and opium. The symptoms were somewhat abated under this treatment, but the strength of the patient was much reduced.

September 5th.

Hauftus effervescens ter in die.

6th. The patient the same way ; there was no rale in the chest this morning when examined.

℞ Pilulæ Hydrarg. gr. iii.

Ext. Opii aquos. gr. $\frac{1}{3}$. M.

Fiat pilul. ter in die sumenda.

Vesicatorium magnum parti dolenti.

7th. Was attacked last night with great dyspepsia, cough very bad ; spitting up *frothy* serum with a pink tinge ; pulse 130, weak ; face livid ; hands cold ; great anxiety ; heart beating in a very laboured manner ; extensive churning sound heard all over the chest. He was ordered carb. ammoniæ, and shortly after leaving him, raving set in, and death soon followed.

Autopsy six Hours after Death.—The right pleural cavity contained about a quart of bloody serum. The posterior portion of the lung was covered with a pretty strong layer of lymph, which was about an eighth of an inch thick, and easily torn off. The same was observed on the parietal pleura opposite to this. The surface of the compressed lung was, as is usual in such cases, wrinkled in many places, a mechanical effect produced by compression. These wrinkles require notice, for in the case before us they imposed on more than one of the spectators, particularly at a part of the posterior surface of the lung, where one of the wrinkles formed, apparently, a deep indenture

into the pulmonary substance, *which indenture containing sero-purulent matter, and covered with a thick layer of lymph, bore a strong resemblance, on a cursory examination, to an abscess.* The bronchial tubes were found to be loaded with a frothy serous fluid, but there was no redness of the bronchial mucous membrane.

The first remark that is suggested by this case, is the tendency which excessive depletion produces to the formation of inflammation. This poor man had been five times bled for a fit of apoplexy, and had been debilitated by various other depletory measures, and in three days afterwards, while lying exhausted and drained of blood, inflammation commences in the pleura, and goes on to a fatal termination unchecked by remedies. Again, another circumstance requires to be noticed, which is, that the nature of the blood and its physical qualities must have been altered by the previous excess of depletion, for we cannot otherwise account for the rather unusual circumstance of the colouring matter being secreted by the inflamed pleura along with the lymph and serum of the blood; in a practical point of view, the sudden occurrence of a churning sound denoting the presence of a serous fluid in the bronchial tubes requires serious attention, for dissection proved that it was not the result of inflammation, but *was produced by a true serous flux into the bronchial tubes*, an event of the most sudden occurrence in the case before us, and which was accompanied by the remarkable rose-coloured serous sputa, which might easily have misled us into the belief that pneumonia existed. Here the colouring matter of the blood presented itself along with the serum, first in the pleural sac, and secondly in the bronchial tubes.

SCIRRHUS OF THE ŒSOPHAGUS, (*two Cases.*)

The two following cases were in our wards at the same time, and afforded a good opportunity of comparing together the symptoms observed in each. In one, Benjamin Spear, we for

a long time thought that the difficulty of swallowing was spasmodic, so completely was the power of deglutition restored (and that, as will be seen from the notes of the case, for many days) by passing an œsophagus bougie into the stomach. In the other, Thomas Berry, the patient could at all times swallow liquids with great facility. He was able to drink a tumbler of water with as much apparent ease as any healthy person ; but soon after gulped up the fluid by mouthfuls ; as the fluid passed readily into the stomach, and was only rejected after arriving there, the diagnosis was rendered very obscure, and I attributed his sufferings to disease of the stomach itself. On this account a trial was not made with the bougie, except once before the man's admission, by Mr. M. W. Murphy, the practising pupil who had the care of this patient while in the Hospital, a gentleman, to whose untiring industry and ability I have been frequently indebted for valuable observations. Mr. Murphy did not succeed in passing the bougie, but as he never before attempted this operation we did not attach a proper degree of credit to this trial.

Altogether I should hope that the account given of the symptoms and post mortem examinations of these two patients, will prove useful in elucidating the diagnosis of stricture of the œsophagus. These cases afford another example of the fluctuating, or even contradictory nature of certain symptoms in different individuals affected with the same disease. It is essentially necessary for the physician to be aware of this circumstance, for it teaches him, that in endeavouring to make out the true nature of any affection, he must refer not to a fixed, but a varying standard of comparison. Whether these variations, in the two following patients, could be accounted for by any differences in the diseased parts observed after death, must be left to the judgment of the reader.

Thomas Berry, æt. 64 ; admitted September 23rd ; ill four months. He states that he had always been temperate and healthy, and that about five months ago he was attacked, after

exposure to cold, with cough, without expectoration, pain in the side, or any other symptom for about a month, when he experienced a slight pain at the ensiform cartilage, which generally came on after eating ; this continued every day, becoming more severe for five weeks, and he then experienced a difficulty of swallowing, which he referred to the seat of pain, where he says his food always stopped for about two seconds, and was then rejected. These two symptoms, viz. pain at the ensiform cartilage, and inability to retain food, have every day become more distressing, and are the only things of which he complains ; took no medicine before admission.

Present State.—Extreme emaciation, and great debility, having eaten scarcely anything for the last two months, being quite unable to retain either solids or fluids ; the latter pass without much difficulty into the stomach, and remain there for about half a minute, but are then gradually gulped up, apparently without an effort. The cough has been very troublesome for the last few days, accompanied with abundant mucous expectoration. Never vomited any black matter, or any thing except what he swallows.

Bowels have been costive since his illness commenced ; frequently for eight days without a motion ; appetite good ; pulse 54 ; abdomen fallen ; no tumour to be felt ; the skin is shrivelled and dry, its elasticity quite impaired ; tongue clean and moist ; skin cool ; sleeps tolerably.

Extr. Conii granum, Syrupi, Mucilaginis, āā q. s. ut ft. bolus. Quater in die sumendus.

26th. Was able to retain the bolus, and also a small quantity of broth ; feels improved ; pain and tenderness in epigastrium diminished ; urine high coloured.

27th. Cough very troublesome, preventing sleep ; abundant sero-mucous expectoration ; no change in the other symptoms.

Vesicat. abdom. Pulv. Conii gr. ii. ter die.

29th. The blister did not rise, though left on for twenty-four

hours; milk now remains on his stomach, but a solid is immediately rejected. Complains of great pain in the epigastrium, where there is also considerable tenderness. He says he knows by the sensation which the food produces when going down, whether it will be rejected or not, and he so accurately foretels this, that many suspect he has a power of bringing it up when he pleases. When it is to come up it excites a kind of spasm, from which he seems to suffer much; pulse 70; cough very troublesome; expectoration copious, yellow mucus, mixed with a great deal of serum: no rale over any part of the chest.

Sinapismus abdomini.

30th. Sinapism produced no effect; took some tea and whey yesterday, which he immediately rejected, and was shortly after attacked with a severe pain about the false ribs, which he attributes to the straining; this, with the cough, kept him awake the greater part of the night.

Turpentine stupe, and afterwards a sinapism.

October 1st. He states, that yesterday evening he felt that "*his swallow had returned,*" and that his "*stomach was opened,*" and immediately ate a large bowl full of stirabout and milk, all of which he was enabled to retain on his stomach. Bowels opened once; all his symptoms are aggravated when the bowels are confined; acetic solution of cantharides to be rubbed to the abdomen.

3rd. The last application caused vesication, and he is to-day much improved, and can retain solids as well as fluids.

Repr.

6th. Ate some bread yesterday evening, but was unable to retain it, and has since frequently vomited; cough troublesome; complains of pain about the false ribs, also in the epigastrium, which is still tender; tongue moist.

Acidi Hydrocyanici medicinalis gutts. iii. ter in die.

24th. No material change since last note: one day he could retain his food, and the next would be unable to do so. Last

night was attacked with severe pain in the right false ribs, which, prevents him from taking a full breath : troublesome cough ; copious expectoration. The whole of the right side is so tender that he cannot bear the slightest pressure : great thirst ; tongue furred and moist ; pulse 56.

26th. The pain last night became so severe in the right side that it caused a kind of convulsion, during which he worked violently for two hours. Tongue furred and moist ; great thirst ; can scarcely speak ; extreme debility ; has not eaten anything for the last three days.

Died on the 27th.

Autopsy eighteen Hours after Death.—The abdomen was considerably distended, though before death it was remarkably collapsed, and tensely concave. On opening the abdomen the stomach and intestines were found distended with air ; and in the latter were hardened fæces. On raising the stomach, the coats were so thin, and so much softened, that the fingers passed through them in every direction ; the mucous membrane was very soft and easily detached. The last two inches of œsophagus were inflamed ; and above this, to the extent of about three inches, was a continuous mass of scirrhous growth, contracting the œsophagus to about the size of a goose-quill ; the mucous membrane above this was thickened and softened, and could be easily separated from the sub-mucous tissue.

Left lung healthy ; the right was connected by strong adhesions to the parietal pleura, in the cavity of which was found nearly a pint of thin fluid, mixed with shreds of lymph, (recent) ; the lower portion of the lung was covered with lymph ; spleen enlarged and very soft. Two of the vertebræ opposite the stricture, presented knobs on their anterior surface. These knobs projected about three-quarters of an inch beyond the remaining surface of the vertebræ ; they were covered with a thin lamina of bone externally, and they displayed a healthy cancellated structure, continuous with the

cancellated tissue of the vertebral bodies. They consisted, therefore, of an exuberant growth of healthy bone, and they each comprised a portion of two contiguous vertebræ. The intra-vertebral substance had undergone a corresponding increase, and was prolonged so as to divide each knob into two portions. It could not be ascertained whether these bony protuberances had any connexion with the production of the stricture. In this case the vomiting or rejection of the food, *after it had passed the stricture*, was a very remarkable circumstance ; it may, perhaps, be explained by supposing that the inflammation of the œsophagus extended to the stomach. The stomach was excessively thin and membranous ; in fact it was, like all the muscles of the body, extremely emaciated. This case was watched and recorded by Mr. James Brady, one of our most distinguished pupils.

The scirrhus mass was in this man rather considerable, and had caused a nearly complete degeneration of all the tissues of the œsophagus. Posteriorly, where it was thickest, it was three-quarters of an inch in depth, and it had evidently arrived at the stage next to that of ulceration : it was not yielding or elastic. These circumstances account not only for the narrowness of the stricture, but for the inflammation of the mucous membrane of the stomach and œsophagus ; on this account too the bougie would not pass.

In all these particulars it forms a strong contrast with the next case, where the morbid tissue was still elastic, and the stricture dilatable and free from inflammation.

Benjamin Spears, æt. 50, admitted into hospital August 29th, 1838. Had been a soldier, and served many years in the East Indies ; of most intemperate habits. Says he has been always healthy, never having jaundice or ague ; never subject to cough or dyspnœa. Says that about a month since, he noticed a slight soreness on swallowing, referred to epigastric region, which continued for four or five days ; when on attempting to swallow a piece of bread, he found it stop at a part

corresponding to about the centre of the ensiform cartilage, and that he immediately rejected it ; that since then he has been unable to retain any thing ; that on its passing down, it is rejected in a few seconds without any effort ; has taken nothing for three weeks. Bowels have been confined ; had one motion each week ; appetite has been bad, and his sleep much disturbed ; has not had cough or pain in chest.

Present State, August 30th.—Great emaciation ; countenance sallow, anxious ; abdomen fallen ; total inability to retain either solids or fluids. Feels, on attempting to swallow, a pain at inferior part of ensiform cartilage, to which he refers the obstruction ; the food is returned without any effort, the diaphragm scarcely appearing to act. On measuring the quantity swallowed, and after its being rejected, it is found increased, appearing to be more than the addition of the saliva would produce. Some tenderness on pressure in epigastrium and right hypochondrium ; has no pain elsewhere ; no tumour ; *no dyspnoea or cough* ; much thirst ; tongue dry and slightly coated. Bowels confined ; extremities cold ; pulse 100, very feeble and small ; respiration 15, natural ; on deep inspiration feels some soreness in right hypochondrium.

℞ Solut. Ichthyocol. ℥ iii.

Tincturæ Opii. gutts. v.

Ft. Enema bis in die injiciendum.

Applic. Emp. Lyttæ Epigast.

The œsophagus bougie to be passed.

31st. Œsophagus bougie passed yesterday by Dr. Collis, *who says he met with no obstruction* : immediately after the passing the bougie, felt some water which he took pass beyond the obstruction ; has taken some whey since, had slight nausea on swallowing it, but it remained.

31st. To get a pint of isinglass and milk.

September 1st. On attempting to swallow a small piece of meat yesterday, felt considerable pain, and rejected it imme-

diately. Is able to swallow and retain the isinglass and milk ; is greatly better.

4th. Has had no vomiting since ; has taken the isinglass and milk regularly. Bowels are confined ; has had no cough ; was seized yesterday with a severe stitch in right side, under mamma, attended with dyspnœa.

7th. Had vomiting yesterday, but was able to retain some of his milk ; is very weak ; the pain in side better ; very little cough. Tongue dry ; pulse 76, very feeble.

9th. Total inability to swallow ; every thing rejected ; refers the obstruction to same place as before ; pain in side better. Pulse 80 ; no cough. Bougie passed without difficulty.

10th. *Retained every thing after passing the bougie* ; has much headach ; the tenderness of epigastrium nearly gone ; pain in side better.

11th. Was seized with severe pain in right infra-mammary region last night, with much dyspnœa and cough ; had no vomiting since.

12th. Pain still very severe ; much cough ; expectoration scanty ; no vomiting.

13th. Pain still very severe ; much cough ; no vomiting.

14th. Pain still severe, preventing him from sleeping ; had no vomiting.

18th. Pain in side still continues ; is very weak ; cough troublesome ; sputa very abundant ; *no vomiting*.

25th. In same way ; *no vomiting* ; expectoration profuse ; pain less severe.

30th. In same way.

October 8th. Mentioned that he had a swelling in perinæum, which was opened by Mr. Crampton, and a large quantity of very foetid, thin matter discharged, from which he found great relief. Cough very severe ; expectoration abundant.

12th. Very weak ; continues in same way ; cough severe ; expectoration profuse, of same character as before.

18th. Expectored in night a large quantity of puriform

matter, very foetid; is excessively weak; pulse 100, feeble and thready; extreme emaciation. Examined in infra-mammary region of right side corresponding to seat of pain, a distinct gargouillement, with cavernous respiration, was for first time audible; pectoriloquy partial; extremities cold.

Died at three o'clock, on 19th.

Autopsy.—Appearance of body extremely emaciated. On opening the œsophagus, all its upper part was found quite healthy, to within three and a half inches of its termination, where a stricture existed, through which the little finger could not be passed, but which admitted a large metal bougie, one-quarter of an inch in diameter. On slitting open the strictured parts, the mucous membrane appeared quite healthy, without any appearance of ulceration; and on dissecting the mucous coat off, the stricture was found to arise from a deposit of a cartilaginous structure in the circular fibres of the muscular coats, which, as well as the longitudinal ones, were exceedingly thin, and scarcely to be distinguished; the deposit was irregular, being thicker in one part than another. The stricture was an inch and a half in length; the mucous glands above the stricture were something enlarged; the stomach healthy, but contracted: and the intestines presented no morbid appearance. Strong adhesions attached the right lung to parietes, which on being torn through the fingers passed into a large superficial cavity of irregular depth, corresponding to the infra-mammary region, where the acute pain was complained of. several crude tubercular deposits existed in different parts of the lung, but none of them in a state of softness; several small calcareous bodies were found in the apex of same lung: left lung was quite healthy.

In this case the stricture was easily dilated; and the operation of passing the bougie produced such remarkable and so long continued relief, that I was led to consider the obstruction as merely spasmodic, induced by a passing or temporary œsophagitis.

PERICARDITIS WITH EFFUSION.

The following case was so accurately noted, and the dissection so satisfactorily explained all the symptoms observed, that I have thought it worthy of publication. It is deserving of notice that here, as in many other instances, increase of energy in the heart's action preceded the appearance of the more characteristic and essential signs of pericarditis, a fact seeming to denote that the disease often commences in the muscular substance of the heart, and from that extends to its investing membrane. Two years ago, Dr. Marsh, Mr. Lees, and I, saw a case strongly illustrative of this opinion. An athletic young gentleman contracted a very acute rheumatic fever from cold; the pulse was very high, the heat of skin excessive, and the pain, tenderness, redness, and swelling of the joints were of more than ordinary severity. He would not allow himself to be bled; we employed an antiphlogistic treatment, and were constantly on the watch to detect the first approach of pericarditis. One night Mr. Lees detected intermission of the pulse; this, in a few hours, was followed by increased strength of the heart's pulsations, and finally pain was felt. In many other instances I have observed irregular action of the heart to be the first signal of the approaching pericarditis: it is of importance to remember this, for it teaches us to attach more value to this symptom as a precursor of inflammation; and besides it proves that irregular and intermitting pulse may, in pericarditis, precede effusion, and do not necessarily arise from the impediment which the latter, when it takes place, must throw in the way of the heart's action.

Mary Kernan, ætat. 10, was admitted into hospital yesterday, October 6th, in a state of collapse, moaning, sighing, and evidently suffering great distress from difficulty of breathing; the pulse could scarcely be detected; her extremities were cold, and considerable tenderness existed over the left side of the chest. Carbonate of ammonia, with calomel, and dry cupping to the painful parts, were ordered; and being this morning more at

ease and less in agony, she gives the following statements as the history of her illness.

Being placed in a draught of air, yesterday week, whilst lying in bed, was seized the following day with shiverings, vomiting, headach, pains in the loins, thighs, and legs, also a beating of the heart so strong as to make her imagine it would at last “thump” through her side, continuing for two days with slight intermissions in its violence ; it was then accompanied with an acute, sharp, lancinating pain in the mammary region, extending to the neck and back, being particularly severe between the shoulder blades and the left arm as far as the elbow, and aggravated by motion, inspiring fully, or muscular efforts of any description. Added to these complaints, there was difficulty of lying on the left side, with shortness of breath, and a hacking, distressing cough, without expectoration ; this, however, she had had for many days previous, without any attendant pain or other untoward symptom. From the chest, the pain seemed to spread, or dart forward to the right side of the abdomen, and from thence over every part of the belly, occasioning more uneasiness than when confined to its primitive seat. Prior to coming in, some purgative medicines were given with slight relief. For several nights past, her sleep has been much disturbed and she now lies on her right side, groaning frequently, and prostrated in strength, so as to be unable to raise herself in bed without assistance. She complains mostly of urgent thirst, a stuffing about the chest, and a “great weight, or heavy load on the heart,” inability to lie on the left side, or sit up from an increase in the cough ; pains in the mammary region, and palpitations of the heart ; when pressure is made over this portion of the chest, much disquietude is produced.

Her countenance is bloated, œdematous, and pale ; lips almost colourless ; skin hot and dry ; breathing rapid and laboured, 48 in the minute ; pulse 120, small, feeble, varying in strength, and intermittent ; tongue furred and clammy.

The left side of the chest to the eye appears fuller, of

larger dimensions, and the muscles, as it were, puffed out ; this is particularly obvious about the nipple ; when measured, no inequality between the two sides can be discovered ; percussion from an inch below the left clavicle, to the lower part of the cardiac region, also laterally over a space of several inches, is perfectly dull ; this is likewise observable over the middle and inferior parts of the sternum, and to the right of this bone, whilst posteriorly over both scapulæ, as far as their spinous ridge and below these bones, it is preternaturally clear. Respiration is exceedingly feeble over the dull parts, but free from rale, and elsewhere very loud. Impulse of heart cannot be felt ; its action feeble, sounds indistinct below the mamma, becoming more audible towards the sternum, and can be heard in the epigastrium. No bruit can be detected. Abdomen full, tense, and much pained by pressure over the hepatic region.

Applicentur hirudines vi. regioni cordis et hypochondrio dextro.
Hab. Pulv. Hydrarg. c. Creta gr. v. ter in die.

7th. Leeches were applied to the hepatic region alone ; she expresses herself as somewhat relieved, and can now lie on the left side without being so much inconvenienced ; slept better, and moaned comparatively little ; pulse very irregular, is full and soft at one time for eight or ten beats, then diminishing in strength, it increases in frequency, to the rate of 120 or 130, gradually vanishes from beneath the finger, and ceases to be felt ; the succeeding pulsations are full and distinct, not more than 88 or 90 in the minute. Respiration 48, still distressed ; bowels opened twice ; tongue loaded and moist. Percussion over the parts noted above remains the same ; on the clavicles of each side it is quite natural. Immediately above the left clavicle there is an evident fulness or swelling of the lower part of the neck, not visible on the right side ; *and on coughing a tumour is brought into view, which disappears as soon as the paroxysm subsides.* Respiration in this part is perfectly dis-

tinct; a wheezing rale is audible in the lower portion of the left side. Heart's impulse and action the same; in the erect posture its sounds can scarcely be detected, but on lying down they are tolerably distinct.

Applicetur Vesicat. Epigastrio.

Repr. Pulv. Hydrarg. c. Creta.

8th. Was very restless the entire night, moaning frequently, and coughing constantly. Her countenance is less swollen; her breathing is more difficult; and she complains principally of the "stuffing and weight about the heart." Pulse remains of the same character, but is not so irregular.

No change has taken place in the phenomena either of the lungs or heart, except that the fulness in the lower part of the neck is more apparent, and the bronchitic rales more distinct in the inferior and middle portions of each lung. Abdomen not so tender, but still swollen; bowels purged.

Leeches were again ordered, and a further attempt made to bring the system under the influence of mercury by inunction and the vapour of a mercurial candle.

9th. Breath is slightly mercurial; appears less affected in her breathing; the respirations continue rapid, 40 in the minute; no alteration in the character of the pulse.

There are now intense cooing and hissing rales in each lung posteriorly, but otherwise no change has taken place in the percussion or respiration. The cough is very troublesome, and attended with a frothy tenacious expectoration; pains increased and palpitations induced by lying on the left side.

Repr. omnia ut heri præscripta.

Applicetur Vesicat. Hypochond. dextro.

10th. Prefers being in the erect posture, being more at ease, less oppressed, and in a great measure relieved "of the weight and load on her heart." Her countenance and aspect generally are improved, but her breathing remains frequent

and laboured; *the pulse is regular, 128 in the minute ; does not vary in strength, neither has an intermission occurred during so many beats.* She is at present sitting up in the bed, and whilst in this posture the pulse was counted.

Percussion over the inferior portions of each lung posteriorly, the left in particular, has lost its tympanitic sound, but retains it at the superior parts. Heart's impulse is still imperceptible, its sounds are distinctly audible along the sternum.

Applicetur Vesicat. lateri sinistro.

Repr. alia.

11th. The pulse again varies in strength, intermits occasionally, and partakes of the description given on the 7th, is 120 in the minute, but she is now in the recumbent posture; passed the night, as heretofore, moaning and in a very restless manner; complains of the oppression about her heart being increased, and refers it to the lower part of the sternum and right side. There is considerable wheezing in the throat; on account of the blister no examination of the chest could be made; pressure over the abdomen produces pain; it is swollen and dull all over when percussed.

Applicetur Vesicat. regioni Cordis.

Rep. ut ulto.

12th. The phenomena remain as before, viz. fulness about the lower part of the left side of the neck, with pure and distinct respiration; healthy sound on percussion over each clavicle, with the natural vesicular murmur; one inch below this, better marked on the left side, extending over the middle and inferior parts of the sternum, anterior part of right side, and a portion of the lateral of the left, a perfectly dull sound is elicited by percussion; the respiration being almost null in the left, feeble but distinct in the right. A very clear sound on percussion in the superior parts posteriorly, with a mixture of bronchitic and crepitating rale in the inferior lobes, and loud respi-

ration, free from rale, in the superior lobes. Heart's impulse and action the same. Pulse much weaker; respiration more frequent, 56 in the minute; breathing free; tongue loaded.

13th. Pulse almost imperceptible; breathing more laboured and distressed; lips of a livid hue.

Died at 11 o'clock, P. M.

Post Mortem fourteen Hours after Death.—External appearance similar to that presented when alive; countenance puffed, pale and œdematous; chest, *particularly left side, full and prominent*, and the abdomen distended and rounded. The same phenomena are afforded by percussion, as noted in the reports during life. The integuments of the chest, as also those of the abdomen, are watery. As soon as the knife pierced the cartilages of the left ribs a gush of straw-coloured fluid took place, and when the sternum was raised nothing but the pericardium could be seen, to such an extent was it distended, as to occupy the mesial line, extending from the diaphragm to within one inch of the fourchette of the sternum, and across to the right side. On removing it from the left cavity of the thorax, the lung was found much diminished in size, pushed upwards, and pressed against the spine and ribs. Having lost a great deal of its natural feel, and appearing like a lung compressed by a pleuritic effusion. The right lung was also affected in the same manner, but in a minor degree. Slight adhesions of recent formation existed between the left lung and pericardial sac, as also between the pulmonary and costal pleura, at the superior lobe of the right lung.

The pericardium itself is increased to at least three times its natural capacity; its exterior highly vascular, whilst its internal surface appears smooth, shining, and covered with a gelatinous kind of fluid, resembling the mucous coat of the stomach, or other portions of the intestinal canal. Its thickness is from three to five lines; but on inspecting the cut surfaces minutely, it is evident this increase is produced by the addition of a false membrane. On the superficies of this membrane are several

patches of apparently coagulated lymph, stained of a purple or dark red colour; differing considerably in their dimensions, and situated in particular near the base of the heart, and that part of the sac in connexion with the posterior surface of this viscus; the larger of these, however, of an oblong shape, about two inches in length, and of a darker colour than the rest, is situated where the anterior part of the heart and pericardium are in contact. Besides these, there are innumerable depressions, or pittings, capable of admitting the end of a probe on the lower and anterior part of this membrane, whilst near the base of the heart and the posterior part of its investing sac, this coating is separated into distinct patches, the serous covering of the pericardium being quite apparent underneath, and presenting its natural glistening appearance.

This false membrane can, with the greatest facility, be scraped off in solid pieces by the nail.

The entire surface of the heart is of a vermilion colour, and coated over with a most beautiful honey-comb, reticular kind of organized lymph, exceedingly fine, but perfectly adherent to the layer of serous membrane covering the heart at the apex.

Advancing upwards or nearer to the base, it is more condensed and compact, seemingly farther progressed in the process of organization, the shreds and interlacing fibres being increased in bulk.

From the quantity of this crimson-coloured network, at the commencement of the aorta and pulmonary artery, it is almost impossible to distinguish between them, so closely are they united together. The under surface of the auricular appendices, and that part of the heart they rest on, are the only portions which do not present to the same degree, and in a slight manner merely, the general aspect described.

Covered in this manner, and to such an extent as the anterior surface is, the posterior is trebly more so, and with a form of lymph more organized, denser and firmer, and from its exterior are three or four appendages, tough, closely adherent to

and evidently taking their origin from the surface of the coagulable lymph.

On the removal of a portion of this coating, the substance of the heart beneath presents a rosaceous hue ; its size does not appear to be much altered, perhaps larger than natural. No examination of the interior. A quantity of the same coloured fluid escaped from the cavity of the abdomen on laying open its parietes ; the liver did not appear increased in size, and its structure was perfectly healthy ; bands of lymph passed between and connected together the visceral and parietal peritoneum, few and slight and not connecting together the intestines themselves. The interior of the intestinal canal was not examined.

Observations.—In the preceding case the following points are worthy of notice :

1st. The great size of the tumour formed by the distended pericardium.

2ndly. The protrusion of the left lung to a considerable extent above the clavicle, forming the tumefaction observed in that situation.

3rdly. The tympanitic sound produced by the close application of the lung to certain parts of the *tense* pectoral parietes.

4thly. The varying states of the pulse, at one time intermittent and irregular, at another, free from these characters.

5thly. When admitted, copious effusion into the pericardium had already taken place, and yet the countenance was pale, and the lips colourless ; there was no suffusion, no lividity, no venous turgescence whatever in the eyes, face, or lips ; and yet her breathing was 48, and the pulse feeble, varying in strength, and intermittent.

6thly. Although it is said in the report, that the left half of the chest did not measure more than the right, yet there was an evident dilatation of the former, exactly corresponding to the distended pericardium, which pushing before it the flexible parietes, formed a well marked and evident prominence. This

likewise rendered the parietes of the superior portions of the left side of the chest more tense than natural, an occurrence sure, for reasons well explained by Dr. Williams, to occasion increased resonance on percussion. *I am not aware that this consequence of pericarditis has been before described.*

ART. XIX.—*Observations on the Exhibition of Mercury in minute Doses.* By ROBERT LAW, M.D., A.M., Physician in Ordinary to Sir Patrick Dunn's Hospital.

A YOUNG man recently came under my care affected with syphilitic rheumatism. His pains were very distressing, and completely interfered with his sleep. He had undergone a variety of treatment, but without any relief. Just before he became my patient, he had taken fifteen grains of blue pill daily for six weeks, without his mouth becoming sore, or his pains experiencing any benefit. I found that while he was under this mercurial treatment, he was neither restricted in diet, nor confined to the house; I determined to try mercury with him, but under different circumstances. He was put on low diet; and in order to insure his remaining in bed, his clothes were taken from him; he was then directed to take two grains of calomel, and a quarter of a grain of opium, thrice daily. He had only taken twelve grains of calomel, when his mouth became sore, and salivation ensued. His pains then began to yield, and soon disappeared altogether. Thus, with twelve grains of calomel, exhibited with attention to circumstances calculated to promote the action of the medicine, we succeeded in accomplishing what fifteen grains of blue pill, exhibited daily for six weeks, (630 grains) but without such attention, failed to effect. We were quite sure when he came under our care that he was not under the influence of the mercury he had previously taken. The result of this case, with many other similar ones, confirmed us in an impression that we had long entertained, that there is not in the *Materia Medica* an agent whose just pretensions are

more compromised by a slovenliness and want of care in its exhibition, than mercury. And although, in some cases, the peculiar circumstances under which the medicine is exhibited, prevent the conditions of restricted diet and confinement being complied with ; and, therefore, the physician prescribes it under disadvantages, of which, although he is aware, yet he cannot control them ; still we believe, from the little importance attached to these conditions when they might be enforced, that most physicians have yet to learn to what extent they modify the action and effects of the medicine. Upon inattention to these circumstances we should charge, in many instances, the complete failure of mercury to affect the system ; and in all, that when the system is brought under its influence, so much more of it is required to produce this effect. An anxiety to ascertain to what extent a due attention to these modifying conditions would influence the results of the medicine, led us to bestow a good deal of pains upon the subject. An hospital, of course, afforded the best opportunity for such inquiry, as there alone could strict attention to directions be insured. To a single result of such inquiry will we advert at present, viz. *the very small quantity of mercury required to affect the system, when exhibited in minute doses at short intervals.* This quantity was much smaller than we could have had any idea of. The first cases in which we made trial of this mode of giving mercury were chronic cases, such as we felt would, without injury or detriment, await the result of our experiment. We made no particular selection of cases, but such as were labouring under affections which we ordinarily treated with mercury. We directed one grain of calomel to be mixed up with a sufficient quantity of extract of gentian to make a mass to be divided into twelve pills, one of which was to be taken every hour. We found, in some cases, salivation produced by twenty-four pills, or two grains of calomel ; and seldom were forty-eight pills, or four grains, required to produce this effect. We would say, that thirty-six pills, or three grains, was the average quantity required to effect salivation. We ex-

hibited blue pill in the same way, and found the mouth to become sore from six grains.

The first case in which I tried this new mode of exhibiting mercury, was that of a woman, (Mrs. Smith,) aged 60, affected with paralysis of the muscles of the left side of the face, and with general weakness of the left side of the body, almost amounting to hemiplegia. She had only taken twenty-four pills, or two grains of calomel, when her mouth became affected. An immediate amelioration took place in her symptoms; and we are quite satisfied that the amount of amendment was fully equal to what would have followed salivation produced by a larger quantity of mercury. The next case was one of hemicrania, or violent pain of the left temple, in a man of thirty years of age. Leeches afforded him slight relief. When he had taken thirty pills, each containing a twelfth of a grain of calomel, his mouth became affected, and his pain ceased, nor did it return. We shall specify a few of the cases, with the precise quantity of the medicine which produced salivation in each.

Anne Carey, aged 46, affected with periostitis of the femur, had her mouth made sore with two grains and a half of calomel.

John Curran, aged 40, labouring under sub-acute rheumatism, with enlargement of the joints of the wrists, was profusely salivated with three grains and a third of calomel: a decided amendment followed.

John Lynch, aged 36, affected with indolent enlargement of both testicles, particularly the right, was ordered a twelfth of a grain of calomel every hour, and to rub ten grains of mercurial ointment on the right testicle every night. He had only taken two grains and two-thirds of a grain, and rubbed twice, when he became salivated. The induration and enlargement of the testicles completely disappeared.

Anne Clare, aged 40, affected with periostitis of the clavicle, was salivated with three grains, and ten-twelfths of a grain of calomel. Salivation continued for a month.

Anne Cullen, aged 19, labouring under syphilitic papular eruption, had been taking the eighth of a grain of the bichloride of mercury, in decoction of sarsaparilla, thrice daily, for a considerable time, without deriving any benefit from it. As she was of a scrofulous habit, we ordered for her the proto-iodide of mercury in the same minute doses as we had ordered the calomel in other instances. Three grains and a third produced salivation, when the eruption immediately began to fade.

Jane Ennis, aged 38, affected with inflammation of the synovial membrane of the left knee joint, was salivated with two grains and a half of calomel.

We anxiously looked out for a case of Iritis to test this method of exhibiting mercury, when one presented itself, in John Gleece, labouring under syphilitic rheumatic pains, with Iritis of the right eye. The conjunctiva was moderately injected, the cornea unusually prominent, and the pupil irregular. We ordered him the twelfth of a grain of calomel every hour. When we paid him our second visit, we found the eye quite clear, no unusual vascularity, and the pupil quite regular. He had only taken eighteen pills, or a grain and a half of calomel; but the gums exhibited no marks of being affected. As the rheumatic pains continued, we determined to persevere in the use of the mercury, and in the same fractional doses, till the mouth became affected. We were surprised to find that this effect was not produced until he had taken one hundred and seventy pills, or fourteen grains of calomel. This seemed to be by much the most refractory case we had met with; however we discovered, that, in order, as he thought, to make assurance sure, instead of complying with our directions of only taking one pill every hour, after the second day, and after experiencing the benefit he received from eighteen, he took forty-eight within twelve hours. So that the case, so far from constituting an exception, by its negative results confirmed our point.

Another case of syphilitic rheumatism, in which the patient equally deviated from our directions, by taking more pills than

we had ordered, required two hundred, or eighteen grains of calomel, to produce salivation. In both these cases, the object we had in view was defeated by the neglect of our directions. For we conceive the efficacy of this mode of exhibiting mercury to depend not only upon its *remora* in the system being insured by the smallness of the dose, but also, upon a succession of impressions being kept up by the exhibition of these small doses at intervals, not so distant as that the effects of the impression be passed away before they be succeeded by another; nor yet so short that the small doses, crowded upon each other, produce but one impression, and that one such as would result from a single dose, equal to the sum of the small doses.

In these two instances the medicine acted upon the bowels. It is worthy of remark, how the Iritis in one of the cases yielded to a very small quantity of mercury without the gums becoming affected, or the rheumatic pains experiencing relief. Can we account for it by supposing that a larger quantity of the medicine was required to correct a more refractory constitutional affection, and for which the system must be effectually brought under its influence, than for the affection of the eye whose cure was within the reach of a smaller quantity, and that not enough to affect the system? The exhibition of mercury in small doses has been adopted by others, at our suggestion, in cases of Iritis, and we have been told that the affection has yielded to a comparatively small quantity without the mouth becoming affected. We would here remark upon what we have observed in exhibiting mercury, that in some instances the morbid action, against which we direct it, seems, if not to cease, at least to get a check almost as soon as we begin to employ the medicine; while, in other cases it is not until the system has been brought under its influence, that any impression seems to be made on the disease. When the disease, so to speak, spends itself in a certain result, thus when inflammation ends in effusion, in this case no effect seems to be produced on the effusion till the affection of the mouth indicates the system being under the influence of the mercury, and then

absorption begins; and this we have not unfrequently observed, when we have employed mercury in cases of pleuritic effusion. But where mercury is administered while inflammation actually exists, and is in progress, it seems at once to grapple with the morbid action. Thus in inflammations of serous membranes, we are sure that we have observed the effectual antagonizing influence of the mercury to exert itself, and stay the inflammation long before the mouth has become sore; so that all morbid action has ceased by the time the mouth has become sore.

We would here advert to the pertinacity with which some diseases resist the operation of mercury, or rather render the system insensible to its operation. Among these diseases we would especially notice puerperal peritonitis and cholera. When, however, the system does yield to the medicine in these diseases, its effects are very violent. We never witnessed such salivation as we have seen to result from mercury in cholera.

We would now return to our original object of recommending the trial of mercury in small doses. We have fairly stated the result of such trial in our own hands, and would ask those who have opportunity to fill up the measure of our experience. We have not yet tried it in primary syphilis. But this we have observed, that we have succeeded in bringing the system under the influence of mercury in a very short time by frictions of ten grains of the ointment. We have found one drachm divided into six parts, and one part rubbed in every night, sufficient to produce salivation.

We have thus ascertained that, both in its internal and external exhibition, less of the medicine is required to affect the system than was usually believed. It now remains to be established, if this effect from a small be equal to that produced by a larger quantity. Of this we feel quite confident, that experience will prove that all the advantage to be derived from the medicine is within the compass of a much smaller quantity than has hitherto been supposed, provided that small quantity be exhibited with due attention to circumstances calculated to promote its effect;

and we would further expect, that this more guarded exhibition of it would save us from the frightful mischief that we sometimes see following it when largely administered ; and which sometimes suggest to us the question, if mankind would not have been benefited by an agent, capable of such mischief, never having been introduced into the *Materia Medica*.

ART. XX.—*An Examination of Dr. Hamilton's Letters in Defence of his Opinions, especially in Reference to the Management of the first Stage of Labour.* By EDWARD W. MURPHY, A.M. M.D., late Assistant Physician to the Dublin Lying-in Hospital.

IN a former Number of the Journal (XXXII.) an inquiry into the management of the first stage of labour was entered upon, in order to examine, and if possible to determine, the value of rules of practice, which being laid down by respectable authorities, were at the same time sufficiently contradictory to embarrass, rather than to assist the practitioner in his arduous and responsible duties. It was my intention to examine the remaining stages of labour in a similar manner, but circumstances have since interfered, which made it necessary to defer doing so. The persevering zeal with which Dr. Hamilton has reiterated the peculiar opinions which were then examined, the controversy which he has *invited* by his animadversions, both in *his work* and in his subsequent letters, upon the practice of the Dublin Lying-in Hospital, as given in Dr. Collins' "Practical Treatise," and the confusion in which the entire subject has been involved, by the novel mode in which it has pleased Dr. Hamilton to defend his doctrines, made it difficult to proceed without either omitting altogether any further notice of Dr. Hamilton's labours, which would be an indirect admission of the justice of his observations, or discussing his arguments in the manner he has put them, and in connexion with the kind of evidence he has advanced in their support, which would be, in fact, little

more than a discussion upon a mode of reasoning, and a species of testimony almost as peculiar as the opinions themselves. To explain and defend an innovation on practice, by a *selection* of cases not treated on the principle advocated, but in opposition to it, is at least new, and it is the more necessary to determine its validity, because a different mode of testing Dr. Hamilton's opinions, by comparing them with the general results of hospital practice, has been objected to by him as that upon which "no logical reasoner could place any dependence." Dr. Hamilton also declares loudly, that his opinions have been misrepresented, and as it would be equally an injustice to him as to myself, not to notice such objections, I have to claim some indulgence in departing from the direct course of inquiry upon which I had entered, in order to investigate their truth. In reference to the latter, it is necessary to state, that in my former paper, allusion had been made to the doctrine which Drs. Hamilton and Burns maintained, of the necessity of limiting the first stage of labour to fourteen hours at farthest, and that in order to do so, artificial dilatation, when the os uteri is lax, dilatable, and thin, may be usefully adopted. In order to shew upon what grounds the assertion was made, I shall take leave to quote their own language. Dr. Burns states: "If the waters have been for some time discharged naturally or artificially, and the os uteri be not effaced, but be lax, soft, and thin, so that it is applied closely to the head and is very yielding, it is both safe and advantageous to *dilate it gently* with the finger during a pain." Again, "In the case I have just considered, I have spoken of the effects of dilating the os uteri, but I do not mean to say, that the practice is useful in such an one alone, for *in most cases of tedious labour it is beneficial*, and as the subject is important, I shall explain my sentiments on it fully." Dr. Burns then, after a discussion on the merits of the practice, and the cautions to be used, adds: "Of the benefit and perfect safety of this practice, I can speak positively, and am happy to strengthen my position by the authority of Dr. Hamilton, who

makes it a rule to have the first stage of labour finished within a given time.”* In a similar way Dr. Hamilton refers to Dr. Burns, not only in his work, but again in his “Refutation.” In endeavouring to assign reasons why his practice is not followed, he says, “the probable reason why my practice in the management of the first stage of labour has not hitherto been adopted by eminent practitioners in Dublin, London, and Paris, is that it has been unknown to them, but Professor Burns of Glasgow, whose talents and experience place him on a level at least, with any of the eminent practitioners in Dublin, London, or Paris, has zealously *adopted* and recommended the very treatment which I have been teaching since the year 1800.”† That the practice which Dr. Burns has zealously adopted, and “found in most cases of tedious labour to be beneficial,” is artificial dilatation, the above quotation, or a reference to the work itself, will sufficiently explain. It is not a little extraordinary, therefore, to find Dr. Hamilton, in a very few pages preceding this last quotation, using the following language in his “Refutation.” “With much regret I have to declare my conviction, that Dr. Collins has either misunderstood or misrepresented (unintentionally of course) my opinions upon this subject; the very title of his paper bears evidence of the fact. By the expression ‘artificial dilatation of the os uteri,’ is plainly implied, its dilatation by mechanical means.‡ And accordingly, in certain cases of hæmorrhagy, in the latter months of pregnancy, the whole profession agree in the propriety of mechanically dilating that orifice, but in my direction for the management of the first stage of labour, the *innovation* which I have insisted upon, is the securing and promoting that

* Burns, pp. 411, 412, 413.

† Refutation, pp. 13, 14; or London Medical Gazette, No. XXXVII., June 10, 1837.

‡ Viz. “dilating gently with a finger if the os uteri be flat, or if it be projecting, by introducing two fingers, and extending them laterally, with gentleness, during a pain.”—Burns, p. 413.

preliminary process to the advancement of the infant, within twelve or fourteen hours from the actual commencement of labour, provided the labour throes continue to recur regularly.”*

Finding Dr. Burns distinctly to recommend artificial dilatation, not merely in one particular case, that in which the waters are discharged early in the labour, but in most cases of tedious labour, and that his authority on this point is quoted with no small degree of confidence by Dr. Hamilton; it is not a little remarkable that the latter thus disclaims the practice altogether, that in fact the nearest approach to what might be mistaken for artificial dilatation, was his advice how to act in one of two *exceptions* to his general rule, as above stated, viz. “that in cases where the liquor amnii is discharged before labour pains commence, it becomes necessary to interfere: generally speaking, venesection from sixteen to twenty-four ounces furnishes the readiest means of promoting dilatation, but cases from time to time occur, when the patient cannot bear the subtraction of blood, and where it becomes necessary to administer an opiate enema. There are also cases where *supporting* (not dilating) the os uteri during a pain is indispensable.”† Such is the treatment given by Dr. Hamilton for that particular exception, in which Burns finds “gently dilating with a finger,” so useful, and in which Dr. Hamilton can only find some cases in which it is indispensable to support the os uteri during a pain. How it could have escaped Dr. Hamilton’s penetration, that it was Dr. Burns, and not Dr. Collins, who had misrepresented him, in thus quoting his authority, he can best determine. It will at least be admitted, that if Dr. Hamilton has been falsely charged with recommending artificial dilatation, there has been quite enough of ambiguity to render misapprehension almost unavoidable. Lest, however, there should be any doubt as to

* Refutation, p. 4; or London Medical Gazette, No. XXXVII.

† Refutation, p. 10; or London Medical Gazette, No. XXXVII.

his meaning, Dr. Hamilton explains, for the benefit of the junior practitioners of Ireland, his treatment, by a case of Dr. Collins. CASE A. No. 210. "When the membranes burst, and the head was low in the pelvis, and the mouth of the womb quite lax and thin, I should have advised pressure on the edges of the os uteri, with *the points of two fingers* during every pain ;"* which would be sufficiently intelligible, had not Dr. Hamilton referred, as an instance of similar practice, to the very passage in which Burns finds "gently dilating with a finger" in most cases of tedious labour to be so very beneficial.

Amidst so much confusion of language, it is not easy to avoid mistake. Dr. Hamilton first protests against the imputation of advocating artificial dilatation. He next specifies where he would "support the os uteri," where he would "press on the edges of the os tincæ," and thus being brought to the very verge of artificial dilatation, we are plunged into uncertainty, by a quotation which seems to imply, that, after all, artificial dilatation is really meant. It is essential to point out these ambiguities, in order to shew the source of those misrepresentations, of which Dr. Hamilton so much complains. In this instance we find artificial dilatation at one time presented to us, but just as we think it within our grasp, it seems we have only to deal with "support of the os uteri," "gentle pressure on the edges of the os uteri," and that under very particular circumstances. Again, having scarcely had time to recover from our blundering, and being taxed for our stupidity, artificial dilatation appears almost as palpably as before.

In truth, the light which these directions, intended as a beacon to young Irish practitioners, afford, is much more like that of a meteor, with which, no doubt, some of them are familiar, and perhaps would require on their parts equal cautions, lest, by following it too ardently, they should be led into diffi-

* Dublin Journal, p. 206, No. XXXVIII.

culties and embarrassments, from which they might not so easily extricate themselves.

The fact of Dr. Hamilton professing to introduce an innovation, has not a little contributed to increase these errors, and if we are to understand Dr. Hamilton as disclaiming the practice of dilating the os uteri by mechanical means, and that this be not the innovation "unknown to eminent practitioners in Dublin, London, or Paris," it is not easy to find out what it is; whether Dr. Hamilton mean a new mode of treatment, by which the dilatation of the os tinæ can be effected in fourteen hours, or an innovation on received opinions merely, which insists that it ought to be *secured* by the ordinary treatment within that time. It cannot be the former, because if we except artificial dilatation, the treatment recommended by Dr. Hamilton is not only not new, but so similar to that of his opponent, that a charge of inconsistency against Dr. Collins is founded upon the fact. Dr. Hamilton, in his usual forcible language, exclaims, "What must be the impression of every reader, when he finds that Dr. Collins declares, after having in such decided language objected to my advice to limit the duration of the first stage of labour to twelve or fourteen hours; that from experience he has found the means recommended by me productive of the best effects." Nay, so strongly does he rest upon this proof, that Dr. Hamilton considers it almost a want of candour in Dr. Collins adopting the practice, "in the utility of which he expresses his disbelief," evidently assuming it to be impossible to find any other utility in venesection, opium, &c., than the limiting the duration of the first stage to fourteen hours, and altogether forgetting that the supposed artificial dilatation was amongst the measures about which Dr. Collins had expressed his disbelief. If his innovation be understood only in the latter sense, and that there be no essential difference in practice, much of the difficulty is certainly removed, and Dr. Hamilton might be permitted to indulge in the originality of opinion, that the obedience of the uterus to his rule of time can, by the ordi-

nary treatment *be secured*, were it not that he attributes (with what fairness may be seen hereafter) every unfortunate result which may happen to cases which transgress his rule of time, alone to the length of the labour. In fact, because other practitioners find such means “productive of the best effects,” only in removing those causes which derange the order of labour, and cannot hope to attain the singular success of Dr. Hamilton in prescribing a given limit to its duration, they are made responsible for any and every unfortunate event which may arise. The results of hospital reports have already been adduced to prove, that when other causes do not interfere, the length of time which the uterus takes to dilate, is seldom attended with injurious consequences. The conclusions then stated, have been since confirmed by the observations of others. Dr. Hamilton has, however, collected some unsuccessful cases which were also protracted, and has presented them as “decided proofs” to the contrary, and in order to render such selected evidence conclusive, he has endeavoured to show “that the returns of large hospitals do not afford data sufficiently accurate for any calculations as to the effects of the duration of labour; and for obvious reasons, which shall presently be examined, “Dr. Hamilton protests strongly against any inference which Dr. Collins and Dr. Murphy have deduced in regard to the effect of the protraction of labour in the Dublin Lying-in Hospital.”* Two such opposite modes of proving a simple fact cannot both be right; and, therefore, a preliminary objection of this kind is too important not to give it every consideration. In discussing a question of treatment, or perhaps more correctly, the validity of a principle, the neglect of which is asserted to be followed by the worst effects, it is at least natural to think that the records of those hospitals in which the principle is never acted upon, and where necessarily these unfortunate consequences must be numerous, would, by the greatly increased proportion of such

* Refutation, p. 17; or London Medical Gazette, No. XXXVII.

cases, furnish decisive evidence of the fact ; and if, on the other hand, that the opposite was found, that it would equally negative the assertion. To this Dr. Hamilton has objected, though the advantage which might be derived from such reports, when faithfully given, seems not to have escaped his acumen. Dr. Hamilton has not, it is true, attempted to establish on the whole evidence which is given, a conclusion in his favour ; but he has found it useful to select from an immense number of cases, those alone which some casualty made suitable, in order to present them as “decided proofs” of an assertion affecting the whole.

Notwithstanding that the reasons which Dr. Hamilton gives for rejecting the collective evidence of hospital reports appears to him quite “obvious,” I confess I find no little difficulty in understanding them, inasmuch as, if some of them were true, they would rather strengthen than take from the force of the conclusion I have derived from them. If the effects of the duration of labour, &c. &c. are influenced by “the previous health of the patient, the treatment adopted, and by other incidental circumstances,” surely such causes will not diminish the dark catalogue which these records must present. If the effects which are predicted to follow from “treatment adopted” on a wrong principle be not found in the increased proportion of unfavourable cases, which the total returns must give, it can scarcely be considered illogical to infer from the absence of such evidence the error of the assertion. The example* which Dr. H. has given of his meaning, in the fact, that in the Edinburgh Lying-in Hospital, during a period of forty-three years, there have been only three cases of ruptured uterus, while in the Dublin Lying-in Hospital there have been thirty-four cases of that deplorable accident,† in no way relieves us from the

* Refutation, p. 17 ; or London Medical Gazette, No. XXXVII.

† In order the better to understand this proportion, it is necessary to observe, that the number of cases delivered within the walls of the Edinburgh Lying-in

difficulty, because, however strongly such a fact testifies to the singular good fortune, which seems always to attend Dr. Hamilton, by which he has escaped these untoward accidents, (the more remarkable as the Edinburgh, like other hospitals, is exposed to the disadvantage of being obliged to receive all cases "sent from various distant quarters," however unfavourable,) such a contrast is no proof whatever of the uncertainty of calculations made with reference to the present question, calculations which demonstrate, that, amongst those cases which transgress Dr. Hamilton's rule, the injuries which are stated as the necessary effect of it, viz. inertia uteri, retained placenta, hæmorrhage, and inflammation, are not in the increased proportion which should follow from the neglect of his "practical precept." If ruptured uterus had been enumerated, or if it was always, or even generally, a consequence of long-continued labour, Dr. Hamilton's forbearance in not pointing to such a difference as an argument, but rather seeking to shew its fallacy, would certainly surprise me ; but as he is well aware that ruptured uterus occurs as often in labours within than above twenty-four hours, and therefore that any calculation proving nothing on either side must be inconclusive, Dr. Hamilton candidly shews the inaccuracy of any inference drawn from the increased number of such accidents in the Dublin Lying-in Hospital, or by contrast imputing "the smallest degree of blame to the medical attendants of that magnificent institution."* Had Dr. H. exercised the same penetration in some of the cases he has brought forward,

Hospital, since the year 1798 to 1834, a period of forty-one years, was 5,198 ; that in the Dublin Hospital in seven years, was 16,414.

* Of thirty-four cases of ruptured uterus in the Dublin Lying-in Hospital, eight were brought into it in that state. Of the remainder the duration of labour was

Under 12 Hours.	Under 24 Hours.	Under 36 Hours.	Above 36 Hours.
17	2	4	3

it would have saved much useless discussion ; but in this instance, had he fallen into the error of assigning ruptured uterus as a necessary effect of the first stage of labour being prolonged, it is clear that the Report of the Dublin Lying-in Hospital would rather weaken than support the inference which I have drawn from them, and that in this respect, at least, Dr. Hamilton would not suffer from their inaccuracy. All such sources of error must increase the list of injuries given in the returns, and therefore, by reflecting indirectly on "the treatment adopted," must support Dr. Hamilton's assertions.

Another reason is assigned by Dr. Hamilton, which, though not more obvious, is certainly much more intelligible, as it would shew that the results of hospital, must be more favourable than those of general practice. He states,* "that pregnant women who resort to the Dublin Lying-in Hospital, and to the Lying-in Hospital of Paris, are much more capable of enduring with impunity a protraction of labour, than women in the grades above them, reckoning from the wives of respectable tradesmen up to ladies of the highest rank," because "the comforts provided for them in those great establishments enable them to bear a protraction of suffering, which would be prejudicial, if confined to their own miserable dwellings, and left to their own scanty means of subsistence." With respect to personal comforts, one would suppose that the inmates of these hospitals have not the same advantages as the grades above them, though it is quite true that the luxurious habits of the latter will often create difficulties in their labour from which the former are altogether exempt. But on the other hand, it must be allowed that "the scanty means of subsistence," and the impure air of "their own miserable dwellings," will produce often amongst the latter a condition of the constitution any thing but "hardy and robust," and one quite as little equal to the endurance of protracted suffering as those enervated by the refinements of

* Refutation, p. 18 ; or London Medical Gazette, No. XXXVII.

fashionable life. There are, however, other disadvantages to which lying-in hospitals are exposed, from which private practice is altogether free ; they have no immunity from those forms of inflammation which are peculiar to hospitals, and which the least abrasion or the smallest injury to the passages, whether caused by a protracted labour or by instruments, could as readily produce in an hospital devoted to midwifery as the slightest wound, or the performance of the most trifling operation is occasionally known to do in those of surgery ; so that the fact “ that febrile and inflammatory affections of a most dangerous nature,” in place of “ not occurring so frequently in patients resorting to lying-in hospitals as in women in private life,” is exactly the reverse. The Paris and Dublin hospitals are also, in common with that of Edinburgh, exposed to a disadvantage of which Dr. Hamilton is perfectly aware, though it is singular that he appears to have forgotten it, until it was necessary to account for the greater number of crotchet cases amongst those delivered in the Edinburgh Hospital, to the “ out patients” delivered, I presume, “ in their own miserable dwellings,” and left to “ their own scanty means of subsistence.” He says, “ the explanation is obvious, deformed women are sent from various distant quarters into the hospital, in consequence of its being evident from their shape that their labour may probably be difficult.”* It might also be added, that in many instances there is not even so much foresight as to do so, but such cases, and many others without any deformity, are sent in, in the very worst stage of an ill conducted labour. I confess, therefore, I cannot perceive how the conclusions I have derived from hospital reports can be affected by any comparison with general practice, and perhaps, if Dr. Hamilton’s “ obvious explanations” why hospitals should have worse cases and more operations than out-door practice, be compared with his “ obvious reasons”

* Refutation, p. 99 ; or London Medical Gazette, No. XLVII. August 19, 1837.

why they should have better, it will be admitted that the only obvious conclusion is, that his objections amount to nothing, and that at least as the errors which might arise from these opposite causes must correct each other, the data afforded by the general results of hospitals are not quite so inaccurate that a logical conclusion may not fairly be deduced from them ; whether such as I have brought forward deserve that character, it is not for me to say, but the very objections which Dr. Hamilton has made convey a tacit admission that they can only be disturbed by attacking the foundation upon which they are established. If I have shown that the casualties which occur and are dependant upon the treatment adopted in these hospitals, however they may appear greater, they certainly cannot be less than the truth, it follows that any argument, in favour of the treatment, derived from the general results, cannot be weakened by any errors which would make these results worse than they ought to be.

I have endeavoured to examine with every attention, and I hope without prejudice, these objections, because they are levelled at what appears to be the only real basis of medical truth, and though conscious of the necessity of avoiding every source of error in any conclusions derived from hospital reports, I am equally convinced that the errors which occur in their statistics are as nothing compared with the mistakes which arise from adopting the mode which Dr. Hamilton seems to prefer. If there be any error more prevalent in medical writings, or more calculated to deprive medicine itself of any claim to the name of science, it is the habit of founding general principles on a few loosely collected facts, in which every thing which might be made to support some pre-conceived theory, is put prominently forward, and any thing of a contrary tendency with equal care concealed ; the natural effect is, that as doctors proverbially differ, and each often as bold in assertion as they are deficient in proof, the press is loaded with a mass of conflicting opinion, and contradictory experience, which is only calculated to produce embarrassment, if not to degrade medicine to the

level of mere empiricism. To avoid such sources of confusion, I gladly availed myself of hospital reports as a test of opinion ; and had Dr. Hamilton, in place of attempting to prove a general principle of practice by a few unsuccessful cases picked out for the purpose, in the same way favoured us with a candid and clear report of the Edinburgh Lying-in Hospital, as a proof of his precepts, he would have conferred a much more real benefit on midwifery than by “ Practical Observations,” which are only the assertion of an experience of which we are denied the opportunity of forming any opinion. When we reflect that Dr. Hamilton professes to introduce an innovation unknown to the profession at large, that he has been for an unusually lengthened period in charge of that hospital, and, therefore, had it perfectly in his power to secure a faithful record of his practice, and above all, that he has been for years instructing the junior part of the profession in these peculiar opinions, he was especially called upon to do so. Had he, like Dr. Collins, given an impartial record of his practice, stating *every fact favourable and unfavourable*, disguising nothing, it would have been in our power to compare fact with fact, and thus to have instituted an interesting parallel between opposite modes of practice, which might have resulted in the mutual improvement of both. Medicine is following too closely on the steps of the sister sciences to be satisfied with mere theory and assertion, when it is possible to have facts ; and when we know the practical errors into which the ablest men in midwifery, from time to time, have fallen, Dr. Hamilton must pardon us, if notwithstanding the respect we are willing to pay to his experience, something more is required than “ the testimony of the public opinion of this city” (of Edinburgh,) to satisfy them on points which can be proved in a far simpler way. It is gratifying, however, to find, by Dr. Hamilton’s last communication,* he has at length perceived the necessity of having such a report drawn up, though it is unfortunate that it did not precede rather than follow an

* Dublin Journal, No. XLI.

over-heated controversy ; much benefit may, nevertheless, be accomplished by giving a faithful return of all the cases, distinguishing such as have been delivered within the walls of the Edinburgh Lying-in Hospital, from the out cases ; the necessity for which is obvious, as we have Dr. Hamilton's admission, that the results of the practice in each case is not the same ; it is essential also to state exactly all the deaths of the mothers and of the children, and to give an accurate account of *all the unsuccessful cases*. Such a report would be extremely valuable, and it is the more to be regretted that it had not appeared sooner. Had it been made the basis of Dr. Hamilton's arguments, it would have been more natural, and certainly much fairer, than to appropriate to himself Dr. Collins's Report. If, in the most chivalrous spirit, Dr. Hamilton "considers himself bound to defend his doctrines whenever they are impugned by practitioners of undoubted respectability,"* he should at least enter the field of controversy on equal terms ; but it can hardly be considered fair, to attack so unscrupulously the practice of the Dublin Lying-in Hospital, while that of the Edinburgh is safely concealed within those "ponderous ledgers," with which, at his suggestion, the Governors have provided Dr. Hamilton. However, as in medical subjects to elicit truth from facts is far more important than to wage a war of opinions, Dr. Hamilton might even be permitted to enjoy the temporary advantage in this way obtained, did not truth itself suffer from the manner in which his facts are stated. He must allow me to protest with equal earnestness against any inference deduced from *selected cases*. To endeavour to disprove one of the most salutary rules of practice, viz. "that so long as the head advances ever so slowly, the pulse continues good, &c. interference should not be attempted, unless the child be dead," by an isolated case,† the very details of which prove it to be an exception, is

* Dublin Journal, No. XXXVIII. p. 202.

† Dr. Hamilton's attempt to disprove Dr. Collins's rule, "that so long as the head advances ever so slowly, the patient's pulse continues good, the abdomen

not very conclusive, but when we find that the case itself does not even come within the terms of the rule, its weakness amounts almost to absurdity. Or, in the same way, to extract from an extensive Report of several thousand cases, given with a fidelity and candour to which Dr. Hamilton himself bears testimony, just such cases as some accidental circumstance or casualty made suitable, and to present them as a kind of *a fortiori* argument in favour of his doctrines, is not a mode of reasoning calculated to carry conviction along with it.

Dr. Hamilton's reasons for discarding the whole evidence of Dr. Collins's Report have been already considered. That which he has offered to justify his selection of a part is almost as convincing. "It cannot," he remarks, "be doubted, that for practical purposes the means pursued in individual cases can alone be interesting to junior practitioners, to whom Dr. Collins professedly addresses his warnings against my practical

free from pain on pressure, and no obstruction to the removal of urine, interference should not be attempted unless the child be dead," by the case of Lady T——, (as given by Dr. Davis,) who in her first labour was delivered in the country, by the forceps, of a dead child; she suffered so much injury of the parts within the pelvis, "that she was never able afterwards to retain her urine." After some years she became again pregnant, and was delivered of a still born child, after a severe labour of eighteen hours' duration, "during which, such was the extraordinary excitement of the heart and arteries, that it was thought necessary to prescribe venesection," which would have been repeated in four hours, were it not for the obstinacy of a young physician who had some influence with the family. She died on the tenth day after delivery; and this unusual train of symptoms may found to arise from the formation of a large abscess "which seemed to implicate all the structures at the superior part of the cavity, and towards the left side of the pelvis, and of which the left ovary, probably dangerously contused during the labour, had all the appearance of being the nucleus." Possibly also the injuries which she received in her former labour within the pelvis, might have caused adhesions about the uterus, and that the new formed structure did not yield so readily to the increased action of the vessels. It is clear, however, that the pulse did not continue good, and is at least doubtful whether the abdomen was free from pain on pressure.

precepts." "Their expected duty is attendance upon individuals, and the history of the cases which occurred in the Dublin Lying-in Hospital, is declared by Dr. Collins himself to have been for their instruction. Accordingly the practice which has been pursued in the cases of protracted labour detailed by Dr. Collins is that ALONE which relates to the present discussion."* In other words, because the details of individual cases are interesting and useful to junior practitioners, and are for that purpose detailed by Dr. Collins, in order "that the reader," by comparing all the details which are given fully in his work, "may form his own conclusions both as to the practice adopted and as to the general result." A few of the worst only are to be attended to, and every other testimony being set aside, a selection of these cases is to be considered a fair epitome of the practice, and by consequence as so many *a fortiori* proofs of the truth of Dr. Hamilton's assertions. Logical reasoners have not hitherto placed much dependence upon general conclusions derived from particular premises ; and even were the selection of cases given in favour of, in place of against Dr. Collins's practice, they would not be sufficient to establish it, if more complete testimony could be obtained, but when such *ex parte* evidence is brought forward as decisive proof of the safety of a practice altogether different, it will be admitted that Dr. Hamilton draws largely on the simplicity of junior practitioners, if he hope to satisfy them with such special arguments. If every one of the cases which Dr. Hamilton has quoted were just as badly conducted as he would wish them to believe, they will not serve his argument, because if the mortality which occurs in an hospital be any proof of the safety or otherwise of its practice, (and I believe it will not be considered a bad one,) the mortality in the Dublin Lying-in Hospital is less than that of any hospital of which the

* Refutation, p. 37 ; or London Medical Gazette, No. XLIII.

records are published,* and therefore the security of its practice being thus established, those cases which we will suppose from one cause or another mismanaged, must be considered not only as deviations from the ordinary treatment, but also as increasing from that cause, which must have been accidental, the very small mortality of the hospital itself. I only put the question

* In the following table is given the comparative mortality, as far as can be ascertained, of the different European capitals. In the Reports of Maternité given, by Lachapelle and Boivin, the deaths of mothers are not given; and in Lachapelle only the general mortality of children. It was, therefore, necessary to refer to the "Compte Administrative," presented to the "Conseil general d'Administration des Hopiteaux," &c. in which the mortality for the year 1822, 1830 and 1831, is stated in the section on "Population." The Report of the Vienna Lying-in Hospital is collected from Boer's "Sieben bücher über natürliche Geburtshülfe;" and Merriman's Table III. That of London, from Merriman, Table IX. That of Berlin, from Busch's Report, in the British and Foreign Medical Review, No. X. In consequence of the different ways of stating the mortality of children, some including premature children, abortions, and putrid children, under one head, which in other Reports are distinguished, the proportion cannot be ascertained.

Capital.	Period.	Total Cases.	Mothers dead.	1 in	Children.		Authority.
					Dead.	Putrid.	
Paris, {	Maternité 1797 to 1809	17,308	700	24	Merriman, Tab. I.
	Do. 1812 to 1820	22,243	967	..	Lachapelle.
	Do. 1822 to 1823	2,682	93	28	104	..	Compte Adminis- trative, 1822.
	Do. 1830	3,086	126	24	94	..	Do. 1830.
	Do. 1831	3,270	256	13	87	..	Do. 1831.
Vienna	18,642	211	93	Merriman, Table III.
Do.	1801 to 1821	26,965	562	47	604	..	Sieben Bücher, &c. Böer.
Berlin	1829 to 1835	2,056	38	54	132	..	Busch's Report in Med. Review.
London	1760 to 1810	10,190	107	95	Merriman, Table IX.
Dublin	1826 to 1833	16,414	164	100	594	527	Collins' Practical Treatise.

in this way to show the fallacy of Dr. Hamilton's reasoning, but by no means as admitting that, even in these few cases, he has proved his position.

Had it been Dr. Hamilton's object (which I am very far from asserting) to depreciate the practice of another by ingeniously contrived misrepresentations, in which to gain credibility, it was necessary to tell the truth, but in which the whole truth should be as sedulously concealed, such extracts would exactly suit his purpose, and the candour of Dr. Collins would give him reason for heartfelt congratulations ; but as it is hard to believe that any physician of respectability could be influenced by such motives, we can only suppose, that "being impressed with the solemn belief that the rules he has suggested are well founded,"* in the fervour of his convictions he did not think it essential to reason at all about them, but that as a practice of a different kind must needs be wrong, it was only necessary to point out any bad cases which could be found, by way of illustration.

Whatever may have been the sources of Dr. Hamilton's mistakes, it is clear that if such assertions were to be received as arguments, or such partial facts as proofs, no statement however extravagant, no innovation however absurd, but might be defended. Perhaps it may be thought unnecessary to dwell on what, to most readers, must appear self-evident, and if the injury were confined to those only who might be deceived by such transparent reasoning, it would be needless to say even thus much ; but an evil of far greater magnitude, (one which perhaps never occurred to Dr. Hamilton,) would be the natural consequence of making such a use of facts. It would discourage any thing like fidelity in published reports. No practitioner, whose practice is at all extensive, can boast of uninterrupted success ; he must have a portion of evil mixed up with the good fortune of his career ; if, therefore, for the conve-

* Dublin Journal, XXXVIII. p. 202.

nience of a theory, his honestly stated experience be mutilated, by presenting a selection of the worst cases which can be found, and that the general results of his practice be omitted, to make room for so distorted a representation, there are few who would have courage to submit to such a hazard. As hitherto, we would still have partial and imperfect records, in which every thing favourable is carefully noted, and any thing of an opposite character with equal care suppressed, and thus all confidence in reports of any kind would be completely destroyed. When we reflect how many theories, *and even innovations*, are poured from all quarters upon the profession, and how difficult it is to find the truth amidst the tumult of so much positive and contradictory assertion, the value of those Reports, which state facts precisely as they are, needs no proof, and, therefore, any attempt made to disfigure them in such a way, is too palpably mischievous not to protest in the strongest terms against it.

Dr. Collins has already very ably exposed the manner in which his Report has, in this respect, been dealt with ; and it is much to be regretted that Dr. Hamilton has adopted this mode of supporting his precepts in preference to the more natural and useful one of detailing his own practice, because, while it has all the odium of making a most ungenerous attack, it has not even the merit of defending his opinions. His reasoning throughout is perfectly inconclusive. As an *a fortiori* argument its fallacy is almost self-evident. It has been already shown, that whether the cases Dr. Hamilton quotes be well treated or otherwise, they can prove nothing beyond the case itself ; because, if mismanaged, they must necessarily be exceptions to the general treatment, which so far from being chosen as proofs, should rather be rejected with suspicion.

But even the cases themselves, though a collection of the comparatively few unsuccessful cases of which Dr. Collins has had the treatment, do not establish the utility and importance of those practical precepts which Dr. Hamilton has so long taught."

Seven cases (A to G*) are given, in his first Letter to the Dublin Journal, No. XXXVIII. to prove the injurious effects of protraction in the first stage; and of these four (A, E, F, G) are almost the only instances, amongst many, in which a protracted case was attended with the injury stated. Seven more (H to N) are quoted to prove that the approach of danger had been totally overlooked, and "that inattention to Dr. Hamilton's first rule, proved fatal to the mother and to the child. "Yet of these five of the mothers recovered; and of the children, but one could have been saved, if it were possible to disengage the hand from its situation, which, though always desirable, cannot be always effected. Five cases (O to S) are selected to demonstrate that the practitioner is imperatively called upon to interfere before danger is threatened to the infant, or symptoms of danger to the mother present themselves. In all of which it is assumed that the application of the forceps was both practicable and easy. The first brought forward, (p. 473, No. 665,) as being so clearly demonstrative of the necessity of its application, is founded on the fact, that no mention is made of disproportion having existed, "and in point of fact," says Dr. Hamilton, "by the appearances on dissection, it was ascertained that none existed." This assertion is made on no other authority than the omission of the disproportion which did exist, and which rendered the crotchet necessary. The appearances on dissection proved that the woman died of puerperal fever, which was then prevalent in the hospital, and has been so stated by Dr. Collins, in pages 23 and 489 of his work. The next (P. No. 10, p. 303) is a case of ruptured uterus, which speaks for itself. In the third, (Q. p. 469, No. 425,) the forceps was tried and failed; and in the two last, (R. p. 472, No. 639, and S. p. 475, No. 674,) it was employed, and both children were still born, though the foetal heart must have been heard previously, as the forceps was not

* These cases are lettered as in the Dublin Journal; the same cases are in "Refutation," and Medical Gazette, but lettered differently.

generally had recourse to, “without regard to the state of the infant.” The remaining eleven cases (T to D, D) refer to the utility of the stethoscope, which is incomprehensible to Dr. H. “By what extraordinary process of reasoning any practitioner, with conviction that there is such an obstacle to the delivery that a living infant cannot be born, should delay the necessary relief to the woman, till by means of the stethoscope the death of the infant should be ascertained, is to me quite incomprehensible.”* The process of reasoning may be thus stated. No practitioner is justified in destroying a living infant, without his having sufficient evidence to prove it actually necessary ; as that necessity must arise from actual danger to the mother, so the evidence must be positive not imaginary ; or, in other words, it must depend upon the presence of dangerous symptoms to the mother, not upon the *conviction* that they would have presented themselves, had not the child been destroyed. The practitioner knowing that he has been sometimes deceived, even in his strongest convictions, and wishing for something more than the evidence of his ideas to justify him in taking such a step, will not interfere, unless that either the child be dead or such real symptoms of danger appear. I will not say that such reasoning will satisfy Dr. H., but it will sufficiently explain the use of the stethoscope. In cases of this description, the child may either cease to exist previously to any dangerous symptoms, or at the same time with them, or subsequently to their appearance. To those of the first class the stethoscope especially applies ; and its value consists in giving an earlier indication of the death of the child than can in any other way be ascertained, and therefore enables the practitioner to effect the delivery *before* dangerous symptoms arise, and thus prevent *any hazard* to the mother. To those of the second class it is even more applicable, because on the first appearance of dangerous symptoms, the child can be removed without any hesitation or

* Dublin Journal, XXXVIII. p. 226.

uncertainty as to its life. To the third class only, are attached the difficulties of practice ; the same difficulties which rendered crotchet operations formerly so much dreaded, and generally so fatal, because practitioners, more scrupulous than Dr. Hamilton, have hitherto felt much reluctance to destroy a living child, and, therefore, suffered the labour to proceed too far, under the impression that the child was living. Such cases are, however fortunately rare ; and notwithstanding Dr. Hamilton's very different opinion, it has been proved, as Dr. Collins observes, that *where the patient has been properly treated from the commencement of her labour*, the death of the child takes place in laborious and difficult labours before the symptoms become so alarming as to cause any experienced physician to lessen the head.

This fact Dr. Hamilton has attempted to disprove, by quoting from Dr. Collins's book two cases. One, (T, p. 300, No. 32,) was that of a woman sent into the hospital from the country in severe labour ; "her countenance expressive of great anxiety, and pulse 120 ; the foetal heart was acting with rapidity." The probability of the foetal heart soon ceasing, and the necessity to use some preliminary treatment to subdue the excitement she was under, caused the delay, in the interval the uterus was ruptured. The other, (U, p. 483, 1091,) was that of a feeble, emaciated woman, who was fifty-six hours in labour. "Uterine action from the commencement until within six hours of the expulsion of the child, *was extremely feeble, with long intervals*. As soon as the pains began to be brisk, the labour proceeded without difficulty ;" nevertheless she never seemed to rally after delivery, and sunk on the eleventh day. After death the bladder was found to be covered with yellow lymph, and opposite the right ischium a portion of the vagina had sloughed. It is also stated, "that there was nothing apparently in the labour calculated to induce so unfavourable a termination." This case was evidently one of previous exhaustion, perhaps from poverty, in which the only delay to the

labour arose simply from the want of pains ; there was no undue pressure, nor any symptom of inflammation or fever ; to interfere in such a case would just as certainly have been followed by gangrene from pressure of the instrument, as what did occur in the passage of the head, which passed through the cavity of the pelvis only in six hours, and in that time caused a slough opposite the right ischium, the gangrene depending upon the atonic condition of the constitution, not the result of previously active inflammation.

Such are the two cases Dr. Hamilton has brought forward to disprove Dr. Collins's allegation. He states, however, " that other cases may be quoted to prove that several women's lives in the Dublin Lying-in Hospital, in whom the labour was protracted, were brought into great jeopardy, while the child, according to the evidence of the stethoscope, continued to live."* Dr. Hamilton has not, however, referred to them, and as he seldom hesitates to quote any case to prove his position, it is not very likely that they would have been omitted ; but he has passed over several cases delivered from the urgency of the symptoms alone, (Nos. 49, 56, 126, 150, 256, 509, 526, 808, 257, 504, from page 462 to 477 of Collins's Practical Treatise,) and what is most singular, four of these cases (J, K, L, M) are for another purpose quoted in the same letter. In this letter also nine other cases are given to prove that the child was not extracted for some time after the foetal heart had ceased, for the very obvious reason, that with " a new method" of ascertaining a fact, the first cessation of the foetal heart was not considered sufficient, nor until after several trials it could not be heard ; of these nine but one died of peritonitis.

In Doctor Hamilton's last reply, when Dr. Collins exposed the weakness of these proofs, Dr. Hamilton observes, " But he (Dr. Collins) has suppressed the important fact, that I have copied nineteen cases of that description, and that eight of the

* Dublin Journal, XXXVIII. p. 226.

women died." We have already examined an assertion made on the authority of two cases, which ten others as clearly contradict. In this list of nineteen cases, there are eleven recoveries and eight deaths, "where the poor women were allowed to suffer unavailing pain for hours after it had been ascertained by the stethoscope, that the infant *had been some hours dead.*" Amongst the deaths are enumerated these two cases (No. 32, T, No. 1091, U,)* which in the former letter were quoted to prove that the patients' lives "were brought into great jeopardy, while the child, according to the *evidence of the stethoscope, continued to live;*" another case of five hours' duration,† (605,) not given in the page referred to ; and one, (665,) of puerperal fever. From the recoveries are omitted Nos. 725, 1053, and 1041, also quoted for a different purpose by Dr. Hamilton,‡ so that even this list of Dr. Hamilton's, when corrected, gives only four deaths and fourteen recoveries, where the infant had been some time dead before extraction.

Sixteen of these cases are taken from Dr. Collins's section on still-born children, in which is given a short outline of such as possessed most interest in 106 cases of *extremely severe labour*. In this outline *every one of the cases in which the mother died* is detailed or referred to, while several who recovered are only numbered. There were altogether seventy-nine crotchet cases and fifteen deaths ; of which three are related in the chapter on "Tedious and Difficult Labour:" the remaining seventy-six are included in this section, of which forty-one are detailed ; and among them all the other deaths (twelve) which occurred. Besides these, are thirty others, of which nine cases are given, and two deaths. So that among 106 cases of extremely severe labour, there are but four-

* Dublin Journal, XLI.

† Dr. Hamilton gives this case as "page 473, No. 605," there must, therefore, be some error ; the only cases in page 473 are 665, and 639, a forceps case.

‡ C, No. 725, in Dublin Journal, XXXVIII. ; N, 1058, and Y, 1041, in "Refutation," or Med. Gazette, No. 47.

teen deaths, or one in seven and a half; while amongst seventy-nine delivered by the crotchet, there are fifteen deaths, or one in five,* in all of which the fact of the child being some time dead, or the urgency of the symptoms, were the motives of the operation. From this section Dr. Hamilton has extracted twenty-seven cases, of which eleven are deaths, and from this extract is formed this last list of nineteen cases and eight deaths.

The reader can thus appreciate its value, by which the mortality caused by the stethoscope is made to appear in the proportion of eight to eleven. Yet Dr. Hamilton can charge Dr. Collins with suppression of important facts, and even "challenge him to shew one misrepresentation of his facts and practice." What Dr. Hamilton means by a misrepresentation may be quite as ambiguous as many other of his opinions, and therefore it is possible that misrepresentation was never intended. Dr. Hamilton may have supposed that because the cases he has been pleased to select are not incorrectly quoted, there can be no cause of complaint. But when it is considered that these cases are quoted to prove general assertions as to the practice adopted in the Dublin Lying-in Hospital, that they are taken from a mass of facts of the most opposite kind, in which every particular which could assist the reader in ascertaining the proper value of each, is given with unusual minuteness, that to the exclusion of every other, the most unfavourable cases are alone selected by Dr. Hamilton, and upon their evidence, a judgment passed upon the whole, it will be admitted that perhaps that such a mode of treating a Report is not a fair representation of the facts and practice contained in it. But Dr. Hamilton has not only *thus* misrepresented the results of Dr. Collins's practice, and failed to establish the utility and importance of his

* It is extremely difficult to ascertain the proportionate mortality of crotchet operations. Dr. Churchill has, however, been able, from researches amongst various authorities, to determine it. He states the deaths as about one in four cases. Dublin Journal, No. XIX., 1835, p. 21.

precepts by the cases which he has given, but in some of them he has obviously misunderstood the details. The omission of the disproportion in one case (O) we have seen to be enough to furnish Dr. Hamilton with an argument; similar instances occur in other places, for instance where the treatment is not mentioned he assumes it not to have been adopted, (C, D,) though Dr. Collins quite sufficiently explained what the general treatment was, nay, on the very strength of the omission, Dr. Hamilton endeavours to make the practice contradict the precept. Doctor Collins on the one hand is charged with recommending treatment, "in the utility of which he expresses his disbelief," and the other, with not adopting the treatment he recommended, because it is omitted in the details of the case, and this because Doctor Collins states "that no means whatever were used to effect the dilatation of the os tinæ," evidently alluding to artificial means.

Again, when it is stated that the head of the child remained stationary a certain number of hours, and in the sentence immediately following, the symptoms which required interference are given, it is at once assumed that the symptoms continued for that period.

Case I.* is a woman who had convulsions. It is stated "that the head made little progress, *still it advanced slightly*, and was pressing on the perinæum;" then follow the symptoms which made excerebration necessary, it was (became) so firmly impacted in the pelvis, and the pressure on the urethra so great, as to render the introduction of the smallest sized catheter impracticable, which was at the same time distended with urine; her pulse was feeble and hurried, and her strength greatly exhausted. Dr. Hamilton remarks, "if the attentions recommended by Doctor Collins had been faithfully observed in this melancholy case it is impossible to believe that the infant could have been allowed to be *for many hours* so firmly impacted," &c. &c.; no such thing

* Dublin Journal, No. XXXVIII.

is stated ; as soon as it became impacted, and was found not to advance, she was delivered.

In case (K)* the head it described as being stationary at the outlet, that the forceps had been tried and had failed, but that “as the patient’s strength was rapidly sinking, and the abdomen had become tender on pressure, delivery was accomplished by lessening the head.” Doctor Hamilton makes it appear that the head was not only stationary, but that all these symptoms were present during the time stated, and is very naturally surprised “that any practitioner could sit by without offering assistance, while the patient’s strength was rapidly sinking, &c. &c.” The very reasons that are given to justify the assistance offered, when these symptoms for the *first time appeared*, and the best proof that can be given that they were not suffered to proceed to a dangerous extent, is that the woman recovered without a bad symptom.

Case (L)† is a similar instance, in which Doctor Hamilton complains, that the child not having made the least progress for twelve hours, her pulse was allowed to become hurried, and her strength greatly exhausted before proper assistance was afforded. The proper assistance was excerebration, which was accomplished *as soon as these symptoms appeared to justify it*. The woman also perfectly recovered.

When the number of cases Dr. Collins has brought forward is considered, the necessity for brevity in each, and “that most of them are considerably curtailed, in order to state the particulars in as condensed a form as practicable,” some errors in sense might be pardoned, and if they appeared to be treated in opposition to the practice stated in the text, the fact of their being thus curtailed would be a sufficient reason to raise a doubt as to their accuracy; but when, without there being any positive errors it appears that the omissions of one case, (O,) an ambiguity in another, (I,) the misconception of a third, (K,) form the ground-

* Dublin Journal, No. XXXVIII.

† Ib.

work of Doctor Hamilton's commentaries, it only shews how completely his zeal in defence of his opinions has obscured from him the most palpable mistakes in the evidence he has advanced in their support.

The manner in which these cases are brought forward, will sufficiently illustrate Doctor Hamilton's mode of establishing the importance and utility of his precepts. To examine each case in detail would lead to a premature discussion upon points of practice not yet touched upon, and would extend these observations far beyond the limits to which they must be restricted. But this is of less importance, as the management of the first stage appears to be the chief ground of Doctor Hamilton's objections. To the mismanagement of this stage, and especially to its indefinite protraction, most of the injurious effects which follow labours are attributed, and the errors committed by the profession in this way are almost universal. "It consists" (says Doctor Hamilton) "with my own knowledge, that many individuals in *the better ranks* in Dublin, London, and Paris have been allowed to be in labour two, three, or four days and nights, and as several of these patients were afterwards under my care, I am quite certain that their sufferings in the capitals alluded to had been owing chiefly, if not entirely, to mismanagement of the first stage."* Let us examine, therefore, the seven cases Doctor Hamilton has brought forward from Dr. Collins's Treatise.

The first is No. 210, (Case A,)† p. 465. One of the two cases, of which Doctor Hamilton observes, "that excepting a case detailed by Professor Davis as having occurred in an English workhouse, the medical annals of this empire do not record two more shocking instances of mismanagement." The case is that of a woman who died suddenly immediately after being delivered. She "was of a most fretful and anxious disposition," and had been in labour from the 20th to the 23rd of February.

* Refutation, p. 15.

† These cases are lettered as in the Dublin Journal, No. XXXVIII, May, 1837.

The action of the uterus throughout was very inefficient; there was no dilatation on the 20th. On the 21st the os tinæ was dilated to the size of a crown; on the 22nd nearly the same, but during that night though "*the uterus continued to act imperfectly,*" it was nearly completed, excepting towards the pubis, where a portion still covered the head of the child, (a fact which Doctor Hamilton seizes upon to attribute the delay to interception of the cervix;) "the pulse after this became hurried; breathing difficult; and great anxiety, with considerable debility." She was then given an opiate, "in the hope that the uterus would act with more effect afterwards;" rest was produced for some time, but when the pains returned, the same symptoms appeared in a more urgent form. "The head was immediately lessened, and *almost every bone removed* before it could be delivered, and even after it was brought down much caution was required to free the shoulder and body." She expired almost immediately, for which no cause is assigned.

On this case Doctor Hamilton observes, "Doctor Collins expresses himself surprised at the suddenness of death; Mr. Travers, who does not practise midwifery, would have told him, "that it was occasioned by the protraction of the pain." Dr. Collins has expressed no surprise about it, he only states, that in the *post mortem* examination "nothing was discovered to account for the suddenness of death." But what must have surprised Dr. Collins, is the manner in which Mr. Travers's authority is quoted. In a section devoted to pointing out the effect of bodily pain and mental impression in certain constitutions, Mr. Travers says,* "Pain, when amounting to a certain degree of intensity and duration, is of itself destructive. Difficult and protracted parturition is every now and then fatal from this cause;" but lest he be misunderstood as attributing such as the necessary effect of protraction, he adds what appears to have escaped Dr. Hamilton's notice, "and even in cases in

* Travers on Constitutional Irritation, p. 65.

which *neither extraordinary difficulty nor protraction* were experienced, a fatal prostration has sometimes supervened, which has admitted of *no other explanation.*" He then proceeds to state other causes, which are known to produce such an effect, such as previous exhaustion, and strong mental impressions of which he quotes two most striking instances,* in neither of which was there any protraction of pain whatsoever. In this case, therefore, where the woman is described to be "of a most fretful and anxious disposition," Mr. Travers would hardly have said in this absolute manner, that the fatal termination was entirely owing to the "protraction of the pain," well knowing that great mental anxiety, constitutional irritability, and such like causes, over which the physician has but little control, and which shew their effects with little previous notice, when they are coupled with bodily pain of long or short duration, will produce such effects. Neither would we deny that there are certain constitutions in which a combination of circumstances will so depress its powers as to render parturition, much more a protracted parturition, a most dangerous addition. They constitute, perhaps, the most difficult description of case that is met with in practice, because the very weakness of the constitution may retard the labour and protract the pain, while the cause of delay being mental influence, the extent of the danger may not be known until the symptoms of "constitutional disturbance suddenly present themselves ; the same may happen in ordinary labour, and even when all pain was over, and every thing as favourable as could be desired, the woman what is called "safe," the practitioner has been surprised by the suddenness with which these symptoms appear, and hurry on the catastrophe. Such may occur, and be a sufficient reason for directing our attention particularly to the study of such cases, so far as they are met with or recorded, but scarcely enough to infer that protraction of pain alone will of necessity create such a case ; still less to offer this, the *only* instance of sudden death out of 264 cases, protracted beyond

* Travers, p. 21.

the period Dr. Hamilton considers safe, as a proof of the necessity of limiting the duration of the first stage of labour. Where, in certain constitutions, different causes may cooperate in producing a fatal result, it is hardly fair to attribute the effect to one cause only, more especially where that cause was in operation in a number of other instances, without any such effect.

This case, like No. 1091, (U,) is an exception to the ordinary cases of tedious labour ; in the one, exhaustion, in the other, mental despondency, enfeebled the action of the uterus, and consequently the labour was protracted, though by no means severe ; in fact, one of the most distinguishing features of these cases was the absence of violent uterine action, the sole cause of the delay being a want of energy in the uterus to complete what it had commenced. The whole object of the treatment was, therefore, to avoid every source of irritation, which, by increasing excitement, would still further impede the function of the uterus. On this point, however, Dr. Hamilton assumes the language of triumph. In this “shockingly mismanaged case,” his objection is, that “instead of assisting the dilatation, an opiate was given.” In order to make this objection intelligible, he asserts the cause of delay to be the interception of the cervix uteri ; though at no period of the labour did the pains become expulsive, and throughout the uterus acted imperfectly ; the head, therefore, could not have been forced down upon the pubis so as to intercept the cervix. Dr. Hamilton has, therefore, assumed a condition of which there is no evidence. In order to promote uterine action, or as Dr. Hamilton would say, “to effect the dilatation,” an opiate was given ; and, perhaps, Dr. Hamilton will allow, that “the only utility of opiates in cases of protracted labour is to suspend inefficient uterine contractions, which wear out the strength of the patient without advancing the delivery, or to render these contractions more powerful, and there are no marks by which the one result

or the other can be calculated upon.”* In this instance the opiate failed in giving the expected relief, and she was then immediately delivered. The better to understand Dr. Hamilton's commentary on this case, it should be observed, that as the cause which interfered with the regular action of the uterus was mental,† so the effect produced on the constitution was without notice; it was, therefore, out of the power of the attendant to anticipate the result, or to be guided by other than the symptoms which were present; no unfavourable symptoms appeared until the 23rd, and then suddenly.

Again, Dr. Hamilton gives this case as being of ninety-six hours' duration, though there was no dilatation of the os tincæ until the 21st, sixty hours before delivery; Dr. Hamilton being evidently “deceived in his estimate of the duration of first stage,” though he is aware “spurious pains are apt to precede true ones, not only for hours but *for days*.” It is also extremely doubtful whether the uterine contractions continued regular for fourteen hours together, so that had the case occurred to Dr. Hamilton, there is little doubt that where the uterus acted so imperfectly, it could easily be reduced the limit he has prescribed for the completion of the first stage.

The treatment of this case, however, seems so valuable an example to contrast with his own, that it is selected to teach the junior practitioners of Ireland what ought to have been done. Dr. Hamilton would have advised pressure on the edges of the os uteri, and when that orifice was fully dilated, which he assures might have been accomplished “certainly within two hours; if there had been no expulsive pains he would have applied the forceps, and thus have saved both mother and

* Practical Observations, Part II. p. 89.

† That Dr. Hamilton is not ignorant, that mental influence will impede uterine action, and be a cause of protraction, may be learned from “Practical Observations,” Part II. pp. 57, 58, in which he controverts Dr. Davis's opinion that it is not.

child." It might have puzzled those to whom Dr. Hamilton addresses himself to explain, how he could accomplish so much under so many difficulties as this case presented. To complete, in little more than two hours, a labour in which the pains were so weak as scarcely to produce any effect upon the os tinæ, and the utmost difficulty experienced in extracting the child, even when the head was broken up, is a degree of skill which would require something more to make it intelligible *than mere assertion*. Dr. Hamilton has consequently given what he intends as a parallel case, in his "Refutation,"* and is to be considered

* NOTE OF CASE (in "Refutation," p. 72 ; or Medical Gazette, No. XLVII. August 19, 1837.)—Dr. Hamilton's patient "was a delicate individual, not twenty-five years of age, and rather under than above the ordinary stature, who has suffered so much during the latter months of pregnancy, that she had repeatedly been confined to bed for days. She supposed herself in labour about three o'clock on Friday morning, and she continued harassed with irregular pains (*which made no impression on the os uteri*) till nine o'clock in the evening, when an opiate was administered, which secured a *good night's rest*. Between eight and nine o'clock on Saturday morning, *true* labour pains commenced, and proceeded with great regularity and frequency till a quarter before two o'clock in the afternoon, when the membranes burst, and a very unusual quantity of liquor amnii was discharged. The head of the infant immediately descended into the pelvis, surrounded by the uterus, although the diameter of its aperture, previous to the discharge of the water, exceeded three inches. Strong uterine contraction continued, and by supporting the anterior edge of the os uteri during a pain, the head cleared that part at a quarter past two o'clock, and completely filled the pelvis. Notwithstanding strong forcing pains, recurring almost every two minutes, there was no progress whatever by half past four o'clock. The forceps was then applied, and assistance was given during every pain. At six o'clock the infant was safely born. At first it showed no signs of life ; and it was found that it had discharged its meconium. It was quickly recovered ; and its head, which was uninjured, measured in the long circumference, making the chin and vertex opposite points, seventeen and a half inches, being two and a half more than the largest circumference described by Dr. Collins."

Dr. Collins does not state the occipito-mental circumference, and might be as well understood to mean the occipito-frontal, more especially as fifteen inches is not by any means the greatest circumference in the plane taken by Dr. Hamilton. It is not stated whether any tumour was formed on the head, if so, -

as an illustration of his meaning ; but, unfortunately, the resemblance is so slight, that it would be a still greater puzzle to find wherein the similitude exists, or how Dr. Hamilton's case differs from one of ordinary labour.

To begin with the points with which they agree. Both supposed they were in labour before it commenced ; both had false pains before labour ; and both slept the *entire night* before regular labour began : but here the parallel ceases. In Doctor Hamilton's case, so far from the uterus acting imperfectly, the pains are described as proceeding with great regularity and frequency ; the waters, in place of escaping gradually from the first, came away suddenly, and in large quantity. In Dr. Collins's case, before any dilatation of the uterus took place, the head is described as being " low in the pelvis ;" while in Dr. Hamilton's, it descended immediately into it, pushing the uterus before it, dilated to three inches, when the discharge of the waters increased the force of the pains, the whole of which was accomplished in five and a half hours. It is added, " strong uterine contractions continued," and were pressing the head into the pelvis before the dilatation was completed, which made it necessary to moderate its force. It was at this stage Dr. Hamilton supported the anterior edge of the os tinæ, in the same way as Dr. Collins would have resisted the descent of the head. When this was done, the dilatation was rapidly completed ; the head filled the pelvis ; and notwithstanding strong forcing pains occurring every two minutes, there was no progress whatever by half-past four o'clock. Dr. Hamilton then interfered with the forceps, and delivered a large infant at six o'clock.

Thus, this case, the whole of which occupied eight hours, until the forceps was used, in which the uterine contractions

Dr. Hamilton is aware, " the swelling of the scalp, in some cases, lengthens the head to the extent of between one and two inches."—*Practical Observations*, Part II. p. 106.

were strong, continued, and effective, is contrasted with one precisely the opposite, where the uterus acted imperfectly, and “all that was wanting was that the pains should become expulsive,” in order to show by a case, in which every thing proceeded with the most favourable regularity, what ought to have been the treatment of one in which constitutional irritability and consequent irregular uterine action conspired to place under the most unpropitious circumstances. Pains so effective might almost raise a doubt whether the forceps was at all required, and had it been in the hands of a young practitioner, in place of Dr. Hamilton's, I am persuaded he would tell him, when the infant remains wedged in the passage, that “young practitioners must be particularly cautioned against the error of not giving a fair trial to the uterine contractions.”* At the same time I mean not to question the propriety of Dr. Hamilton's assistance, knowing how difficult it is to judge of all the circumstances which might influence the conduct of the attendant in the treatment of a case, from the recital of its leading characters. If, however, an opinion were to be formed from Dr. Hamilton's statement, the reason which he gives to prove the propriety of his interference is not the most satisfactory. Dr. Hamilton remarks, “one remarkable feature of this case, which cannot fail to strike intelligent practitioners, is, that the infant had discharged its fæces, while the great size of the head having filled completely the pelvis, prevented the circumstance from being known, and therefore if the infant had remained a few minutes longer its death was inevitable.”† It must also strike the intelligent practitioner, that the forceps employed for one hour and a half compressing such a head would be a very likely way to produce such an effect; besides it is not probable that the forceps could be applied without moving the head from its position, and still less that some meconium would not have es-

* Practical Observations, Part II. p. 129.

† Dublin Journal, XXXVIII. p. 222.

caped if at all pushed back, had the child been previously in danger. Notwithstanding, therefore, that it is quite evident a few minutes longer delay in the delivery would have caused its death, it is at least doubtful what caused the danger. Had Dr. Hamilton tried the new method, perhaps the stethoscope would have given him more certain information.

It is much to be regretted that this case also fails in explaining Dr. Hamilton's ambiguity as to his mode of effecting the dilatation of the os tincæ. When strong pains force the head down so as to compress the cervix uteri against the pelvis, all admit the necessity of preventing it doing so. Pressing on or supporting the anterior edge of the os tincæ would of necessity act against the head, just in the same way as if two fingers were applied to the head itself, and the difference in both cases, where the uterus and not the fingers effect the dilatation, is so slight as to be immaterial; but when the practice is applied to a case where there are no "strong forcing pains," and the fingers are, as it were, to supply the place of the uterus, it in no way assists us in understanding how it is to be done; and yet because Dr. Hamilton assisted the dilatation by resisting the effect of "strong uterine contractions," he complains that the same was not done where there were none to resist.

It is one thing to criticize a difficult case, it is another to conduct it through its difficulties. In this case the difficulty consisted in the obvious fact, that there were no symptoms to indicate danger to the patient, until those which were the effect of a cause which could not have been known, suddenly presented themselves. Being extremely fretful and anxious about herself, to have teased her with attempts to effect the dilatation in the way Dr. Hamilton describes, when there was no corresponding uterine action, would in all probability have hastened the catastrophe. Its fatal termination, however, has given Dr. Hamilton an opportunity of proclaiming, not in the mildest terms, the injurious effects of neglecting his precepts. The case also presents a fair specimen of these his "decided

proofs." The only instance (among 264 cases) of sudden death is chosen to prove an assertion which applies to all protracted cases, and thus, on a new principle, is the exception made to prove the rule. The case itself, exception though it be, can only prove what is asserted of it, by omitting every other cause known to produce such an effect, though the existence of such a cause is stated; and lastly, the cause of the delay being assumed, it is asserted that both mother and child would have been saved by another mode of treatment, upon the evidence of its success in a case of a totally different description.

B, (No. 608,) is a case in which "the labour pains also were very tardy and feeble, producing much irritation, without *causing any dilatation of the mouth of the womb*. In this state she remained for thirty hours." Rest and a cessation of these pains were procured by opiates, and at the expiration of fifty-three hours she was delivered of a still-born child. Dr. Hamilton is aware "that young practitioners may be deceived in their estimate of the duration of the first stage, especially in cases where the woman has had a family, for spurious pains are apt to precede the true ones not only for hours but for days." In this case such pains lasted for thirty hours; the labour, therefore, reckoning (according to Dr. Hamilton's rule) from the recurrence of the pains, was exactly (53—30) twenty-three hours; the membranes are not stated to have been prematurely broken, and therefore any injurious pressure on the navel-string is doubtful; yet such is the case Dr. Hamilton brings forward to attribute the death of the child (which might be caused in other ways) to the length of time the navel-string was suffering injurious pressure.

C, (No. 725,) is a case *reported* to have been sixty hours in labour before admission; the os uteri little dilated, and the head high up in the pelvis; the pains continued constant for twenty-four hours after she came in, yet the labour made little progress; the mouth of the womb was rigid, jagged, and had the feel of cartilage. The child being dead, the head was les-

sened, and afterwards brought down. Here is a case of rigid os tincae, which Dr. Hamilton remarks, "if there was no other, ought to have roused the attention of the officers of that noble institution to the injurious consequences of allowing the first stage of labour to go on to an indefinite extent." It would have been most desirable if such cases could equally excite the notice of Dr. Hamilton, because though very positive as to the effecting the dilatation of the os tincae within fourteen hours, he gives no directions whatever beyond the treatment commonly adopted for accomplishing it in these instances. The os uteri was not lax, dilatable, or thin, neither was the cervix intercepted; dilating, or rather supporting, the uterus, was therefore out of the question. Whether venesection was used in this case is not stated, but that is no proof that it had not been employed, and the experience of similar cases assures me that it affords no certainty of limiting this stage. I would gladly therefore be informed, how is the dilatation of a rigid, not to say scirrhus, os tincae, to be secured within fourteen hours? or how Dr. Hamilton would have saved the infant in this case? The mother perfectly recovered.

D, (No. 1038,) is another case in which the "uterine action was feeble, and continued so for seventy-two hours after she came in," which prevented its complete dilatation before the foetal heart ceased; as the pulse became hurried she was delivered by the crotchet. Peritonitis appeared on the third day; she died on the sixth. After death a small opening was found in the lip of the uterus, and another in the vagina. Here also, Dr. Hamilton assures us, "if the os uteri had been supported in proper time, and other resources for facilitating the dilatation been employed, the probability is that both mother and child would have been saved." It is, however, only a "probability," and when Dr. Hamilton doubts, the probability, it is fair to infer, lies the other way. We have already had some difficulty in understanding how supporting the os uteri will supply pains. And in this case the want of effective labour, with contracted

pelvis, left the alternative either to cut it short by perforating while the child was living, or to have suffered it to proceed until there was something to justify interference. The latter was adopted.

The remaining three cases, (E, No. 110, F, No. 130, and G, 129,) which are quoted to prove that irregular contractions of the uterus and hemorrhage are the consequences of protraction; Dr. Collins has already shown that they proved exactly the reverse, and that such effects hardly ever follow from protraction of labour.

Of seventeen cases of irregular contraction, E, No. 110 is the only instance in which the labour exceeded twenty-four hours. Of forty cases of retained placenta, F and G, with two others, make just four cases which passed that period. To this unanswerable argument Dr. Hamilton replies, that "in all these four sentences I have used the word *may*, which I need scarcely observe Dr. Johnson defines *to be possible*; if the word *must*, instead of *may*, had been employed, Dr. Collins's inference would have been correct." Dr. H., in his "Refutation," p. 7, sums up these several injuries, headed 1st, 2nd, &c. in these words: "In other words my conviction was, that although there be no injurious pressure on the person of the infant, (thus excluding Cases B and C,) nor on that of the parent, (as Case D,) the protraction of pain above a certain number of hours *must* occasion more or less exhaustion both of the sensorial and muscular powers, (giving rise, of course, to hæmorrhage and retention,) and *must* necessarily influence the subsequent process of delivery." Such is Dr. Hamilton's explanation of his doctrines in the London Medical Gazette;* no wonder, therefore, that Dr. Hamilton's transcriber should have inadvertently inserted the word *necessary*. Dr. Hamilton's admission, that these are only *the possible* effects of protracted labour is important, because while few would deny that such effects *may* happen in long continued labour, it

* London Medical Gazette, No. XXXVII.; June 10, 1837.

is perfectly untrue that such are the *necessary effects* of protraction, and therefore these cases thus brought forward by Dr. Hamilton do not “furnish the *most decided proofs* of the injurious consequences of the indefinite protraction of the first stage of labour,” as he “flatters himself” they do. That they are not even the probable effects, may be readily collected from facts which have since been brought forward. Since by Dr. Hamilton's exertions the attention of the profession has been roused to the subject there has been the most impartial, and therefore the strongest testimony against Dr. Hamilton's assertions.

Dr. Churchill, in his second Report of the Western Lying-in Hospital,* has put Dr. Hamilton's assertion to the test. Twenty-one cases of labour have been carefully noted, in which the first stage was twenty-five, thirty-five, forty-five, and ninety-five hours in duration; and the result was,

1st. That all the children were expelled alive except two, one of which was premature, (six months,) and one presented with the funis prolapsed.

2nd. Neither flooding, retention of the placenta, fever, nor inflammation happened in any case; on the contrary, every one of the cases recovered as well as after an ordinary labour of twelve hours' duration.

Dr. Beatty (an equally impartial authority) has also made Dr. Hamilton's precepts on the duration of labour the subject of observation. In his second Report of the New Lying-in Hospital,† he gives sixty-nine cases, in all of which the labour exceeded twenty-four hours, and in some the duration was seventy-two, eighty-four, ninety-six, and one hundred and thirty-six hours; and of the whole number but one mother died—of ruptured uterus. Of nineteen children still-born, but nine suffered from the severity of the labour. Dr. Beatty remarks, with much justice, “From this record I am justified in saying, that protraction of labour beyond twenty-four hours is not *per se*

* Dublin Journal, No. XXXVIII.

† Ib. No. XXXV.

productive of those injurious effects described by Dr. Hamilton, and that many women may be allowed to go beyond that time with safety to themselves and their offspring."

If, in addition to the testimony thus offered, the very small mortality of the Dublin Lying-in Hospital is considered,—a mortality, there is reason to suspect, considerably less than even the Edinburgh Lying-in Hospital, it will not be difficult to decide as to the safety of the practice adopted, or to appreciate the justice of statements which have no other foundation than a recital of the comparatively few instances in which a practice, generally successful, was attended with an opposite result. If further evidence were required, Dr. Hamilton himself might be quoted as a conclusive authority on the question. It will be conceded that he has shown too little favour either in the cases he has selected, or in the commentaries he has appended to them, to suspect him of omitting any cases which might be bad enough to be "a proof." It is fair to assume that he has advanced the whole, or nearly so, of those which by their unfavourable termination could be made to reflect upon the practice of the Dublin Lying-in Hospital; and when we find that after the most anxious scrutiny, Dr. Hamilton has been able to collect about thirty-four questionable cases amongst 16,414, to support his objections, what stronger evidence can be given of the security of the practice to which he has objected?

In making these observations upon Dr. Hamilton's letters in defence of his opinions, or perhaps, more correctly, upon his attack on the practice of the Dublin Lying-in Hospital, I have endeavoured to point out the ambiguity of Dr. Hamilton's language, the fallacy of his reasoning, and the contradictions, and even inaccuracies, into which he has been drawn by an attempt to draw a contrast, which some might call invidious, but which all would admit to be unnecessary, between his own and Dr. Collins's practice. In doing so, I have anxiously sought to examine Dr. Hamilton's statements dispassionately, and as far as possible to avoid the influence of a controversy, which seems

rising to a temperature almost dangerous to approach. I should have even gladly declined a very disagreeable task, were it not that being engaged in investigating the subject of labour, especially in reference to *opposite opinions* of practice, it would be an injustice to Dr. Hamilton to leave unnoticed those which he has offered; besides that my silence upon the charges which Dr. Hamilton has made relative to the practice adopted by Dr. Collins in the Dublin Lying-in Hospital, might be interpreted as an admission of their truth, particularly as having been one of the medical officers of that institution, I had personal opportunity of judging of the facts. The whole subject also has been so completely involved in irrelevant matter, that it was necessary to examine it separately. The controversy which has arisen from it commenced with one misunderstanding, and seems likely to terminate with several; it was not therefore very easy to follow the real question through so many intricacies, and it may serve, perhaps, as an illustration of the inutility of controversy in medical subjects, that amidst much apparent opposition in practice, much assertion of an innovation, and statements of many injurious effects from neglecting to adopt it, there is after all but little real difference in the treatment itself.

If some allowance be made for those errors which arise from the heat of controversy: if we suppose Dr. Hamilton, on the one side, misrepresented, as advocating artificial dilatation; that Dr. Collins, on the other, is misunderstood in being supposed to use no treatment whatever for the first stage; and that the treatment of both be fairly compared, we may say in the words of Dr. Hamilton, after quoting Dr. Collins's treatment from the first stage, "If these be not the very measures, with the exception of nauseating doses, which I (Dr. H.) have suggested for the certain cases of protraction of the first stage of labour, and against which Dr. Collins has so strongly objected, I do not understand the English language."* In truth, the dif-

* Refutation, p. 23; or Medical Gazette, No. XXXVII., June 10, 1837.

ference consists in the assertion of what is to be effected by it ; Dr. Hamilton making it essential to have this stage completed within fourteen hours, “ provided the labour throes continue to recur regularly.” While Dr. Collins, with many others, altogether objects to be guided in the treatment by a scale of time. Even on this point Dr. Hamilton is sufficiently ambiguous, to make it doubtful whether the difference as to the duration of this stage is so great as it appears to be ; Dr. Hamilton acknowledges that many mistakes have been made respecting it, and it could hardly be otherwise from Dr. Hamilton's mode of reckoning. Dr. Hamilton's ambiguity has been already stated in my former paper, it is unnecessary here to repeat it, more than to mention the fact, that a large class of cases generally given as of very prolonged duration, would, according to Dr. Hamilton's calculation, be very much curtailed. The experienced physician will, therefore, find but little novelty in Dr. Hamilton's practice, however he may doubt the correctness of his opinions.

But the great evil to which Dr. Hamilton's precepts are calculated to lead, is the encouragement which the inexperienced receive in adopting a meddlesome midwifery. It is easy to understand the anxiety which the junior practitioner endures in the conducting of a tedious labour, how many influences act upon him which urge him to interference ; with how much difficulty can he maintain his self-possession, when surrounded by friends anxious for the safety of his charge, and perhaps interpreting his passive watchfulness as the effect of ignorance. It is under such circumstances that suggestions, coming from a quarter where experience is to be expected, become particularly dangerous. If it be the management of the first stage that is to test his competency, when he is told that the completion of it is to be *secured* in fourteen hours, or if the membranes are previously broken, in six or eight hours, how often will he interfere, “ lest the patient's health should suffer,” and his patient suffer from his interference.

Or again if it be the treatment of a laborious labour, when he is assured,* “that the forceps, *if properly employed*, can do no harm,” while *by diminishing* the bulk of the infant’s head, (an effect which Baudelocque† found he could scarcely accomplish even with the French forceps) it enables the practitioner to lessen as well as to shorten the sufferings of the poor woman. “That if the infant *becomes wedged in the passage*, and be within reach of the forceps, the practitioner ought to interfere, before there is a probability that the pressure may destroy the infant’s life, and before any untoward symptoms threaten the mother.” What must be the effect of such precepts to those who are ignorant of the injuries which the forceps is known to produce? Injuries of which the case of Lady T——, as quoted by Dr. Hamilton from Dr. Davis, is but one of many instances. Is not the young practitioner here encouraged to use the forceps under the most difficult circumstances in which it could be employed? where his attempts to compress the head of the child

* Refutation, p. 32; or Medical Gazette, No. XLIII.; July 22, 1837.

† Baudelocque has put to the test of experiment this asserted power of diminishing the bulk of the child’s head, which he attributes to this cause: “Ils évaluent le degré de compression que la tête éprouve entre les serres de l’instrument, par le degré de force qu’ils emploient pour la comprimer et l’extraire; par l’écartement de l’extrémité des branches qui est au dehors, et le degré de rapprochement qu’elles éprouvent dans l’opération, ou l’étendue qu’elles parcourent pour se mettre en contact.” Nine experiments were tried on still-born children to compress the head, both in tranverse and longitudinal diameters, with three forceps: “De la meilleure construction et de la meilleure trempe.” Such was the force used that one was bent, and though from the absence of the circulation in the brain the head must have been more compressible than in the living child, the result was “que cette réduction ne sauroit être, en aucun cas, aussi grande que des accoucheurs l’ont annoncée et qu’elle ira difficilement et bien rarement au-delà de quatre à cinq lignes lorsque l’instrument agira sur les côtés de la tête.”—*Baudelocque l’Art de Accouchmens*, pp. 17 to 21, vol. ii.; Paris, 1822.

When such was the effect of the French forceps on a dead child, used with so much force as to bend it, the reader can judge the degree of diminution which the English forceps could produce on a living child, to say nothing of the danger to the child itself.

must to an almost certainty destroy it, to say nothing of the risk which the mother incurs from the pressure of the instrument applied to a head already *wedged* in the passage ; and this is to be done “ whenever there is decided evidence that the natural powers are inadequate to accomplish it (the delivery) with *safety* to the parent,” which Dr. Hamilton asserts can always be ascertained within the remaining twelve hours that follow the completion of the first stage, which is tantamount to authorizing so dangerous an interference both to mother and child, if the second stage of labour should exceed that period.

It is to be feared, that the young practitioner being guided by such precepts, would soon find that he had not employed the forceps properly ; and the important fact could not be too strongly impressed upon him, that Dr. Hamilton, in fifty years' practice, where he had charge of the patient from the beginning, only used the forceps thirty-five times, not once in the year.

Lastly, if it be a case of protracted labour, where interference becomes necessary, and the practitioner has to decide on the instrument he should use, he receives from Dr. Hamilton an astounding doctrine, which all the explanation which has been given of it, in no way alters, and is expressed in the following passage : Dr. Hamilton “ cannot imagine a case of laborious labour, which had been much protracted, where the knowledge of the state of the infant can be necessary to regulate the practice. If the circumstances permit the safe use of the forceps, that instrument should be employed whether *the infant be dead or alive !* Or, on the other hand, if from some previous mismanagement or other circumstances, it would be unsafe to use that instrument, it ought not to be ventured upon, even though the infant be alive.” That is, if circumstances permit the *safe* use of the forceps, (we have already seen what Dr. Hamilton calls a *safe* use of the forceps,) though the child be dead, it should be employed ; and, on the other hand, if it be unsafe,

that the life of the child is a matter of no import in determining the urgency of an operation when the crotchet must be used, the fact of the labour having "proceeded for a considerable time," being sufficiently urgent to secure the safety of the woman and to destroy her offspring.

It is these considerations which make Dr. Hamilton's precepts most dangerous to the inexperienced ; and while the fact of their being in direct opposition to general experience demands the strongest evidence of their truth, the danger lest they be misunderstood, and abused, requires the utmost caution in stating them. It is to be regretted that Dr. Hamilton has not perceived the necessity for either the one or the other, and it is the more essential to warn junior practitioners of the errors into which they may be led by their adoption. Such, however, has been the ambiguity in Dr. Hamilton's language, and so often has his meaning been misunderstood, so loud has been his protest against misinterpretations, misrepresentations, and mistakes, that though the words seem plain, and the sense is apparently clear, I almost doubt whether the work itself may not be a misrepresentation of his opinions on the subject of labour, at least in the sense in which it would be most usually understood.

If it should be that I have, in common with others, misrepresented Dr. Hamilton, I have only to assure him that it has not been intentional, that I have used every possible caution to avoid any erroneous quotations, and if from any obtuseness I have mistaken the sense, nothing would give me more pleasure than to find myself deceived.

ART. XXI.—*On the Position of the Placenta or Afterbirth in the Womb during Pregnancy, and on the Manner the latter Organ expands therein, as also of its subsequent Contractions in the Process of Parturition.* By HUGH CARMICHAEL, A.M., Member of the Royal College of Surgeons in Ireland, and one of the Surgeons of the Coombe Lying-in Hospital, Dublin.

I HAVE been frequently led to imagine, that the relation which exists between the placenta, or afterbirth, and the womb during certain stages of pregnancy, was not fully understood by writers on midwifery, and that either erroneous opinions were entertained as to the nature of the dilatations and contractions of the latter, or that something as yet remained to be known respecting the former, in order to explain how its functions could remain uninterrupted throughout pregnancy, and in the process of these contractions for the expulsion of the child.

The ideas which occurred to me in the consideration of this subject, further induced me to think, that the latitude also which has been given to the position of the placenta in the womb, namely, that it may be affixed to its superior extremity or fundus, as it is termed, or any other part, (the mouth and neck excepted,) and with safety to the child, was likewise erroneous; and that such an extended scope of attachment but ill accorded with that accuracy and perfection which mark the machinery of human generation in all its minutest particulars.

Impressed with this opinion from reflection on the subject, my attention was particularly directed to these points in four dissections which took place at the Coombe Lying-in Hospital, where I had an opportunity of inspecting the contents of the fully pregnant womb, undisturbed and in their natural position, in order to ascertain if any light could thereby be thrown upon what appeared to me to be as yet unexplained; and the result of these dissections, with other facts which I shall mention in

the sequel, have brought me to the conclusion, that not only are there grounds to question the present received opinion, as to its proper position towards the middle and close of gestation, but that the mode in which the womb expands as pregnancy advances, as also that whereby its propulsive contractions take place in the process of parturition, are not properly understood.

These dissections, with the exception of one of them in the eighth month, took place in females who had expired when on the eve of labour, from various causes, and, therefore, most favourable for the elucidation of what I sought to investigate; so far as dissections could assist me in so limited a number of cases.

In order to the perfect understanding of what I have to offer on the subject, it will be necessary, concisely, to make some preliminary remarks upon the nature of this body, the office it is destined to perform in generation in the human subject, and the necessity there exists for the performance of that office, uninterrupted, up to the completion of birth; although perhaps to most readers, the opinions connected with these different matters are already well understood.

It appears then to be a fixed law of all animals that particular changes should take place in the blood at certain stages of its routine through the frame, and so necessary does this change seem to be, that life itself is subservient to its performance, death inevitably occurring with its suspension.

Subsequent to birth this change, in the higher animals, is wrought in the lungs, by the action of the atmosphere operating on the blood in the air-cells, by means of respiration; but previous to that event, inasmuch as this function (respiration) cannot possibly be performed by the infant in the mother's womb, a provision or substitute is made for that purpose; the placenta, or afterbirth, is created and affixed thereto, endowed with powers capable of effecting the necessary change, and the blood of the infant, vitiated, or in some way altered in its circuit

through the system, is conveyed to it by one series of vessels of the umbilical cord, or navel-string, where the regenerating principle, or some change having taken place, it is returned by another series of vessels of the same cord, again to go the rounds of the infantile circulation, dispensing its nourishing qualities throughout the frame. This power in the placenta, of course, is dependent upon its connexion with the mother, and together with the extent of life with which it is endowed, exists only so long as it is attached to the womb.

Besides this, other offices are assigned to it ; it is stated to be the medium whereby nourishment is supplied to the child, and to which it is indebted for its growth. This latter office, however, from its nature, would admit of suspension for a considerable time without any injurious consequences to the child ; but the one it performs as regenerating in some manner the blood, or whatever is the change produced on it, and for which purpose it is conveyed thereto by the navel vessels, is so analogous to the respiration of after-life, and its continuance, therefore, so essential to existence, that an interruption between it and the infant for a very short period, indeed, must necessarily be attended with fatal consequences.

From this short view of the matter, we may readily perceive how important this organ is to life before birth, just as much so as the lungs are after it ; the continuation of its functions in full operation being indispensable up to, or very nearly up to, the first act of respiration ; in a word, no interruption can take place to both ; when one ceases the other must immediately begin, neither can one abate or diminish in its action till every thing is in readiness to permit of the commencement of the other, consistent with the life of the child.

That the placenta performs the first of these offices, or something similar to it, we have abundance of testimony ; but for the purpose of elucidating the fact, we need not go farther than the result of compression of the navel-string, either when it de-

scends before the birth, or in breech or foot presentation ; how invariably does it not happen, that when the pressure is sufficient to interrupt the communication between it and the child for even a very short period immediately preceding the birth, that the latter is born perfectly dead, though we have every assurance of its existence previous to this occurring. The fact is likewise proved by the quickness with which the death of the child takes place after that of the mother, during pregnancy. In two of the dissections above alluded to, I had an opportunity of removing it in less than fifteen minutes after the death of the mother, yet in both cases it was found perfectly dead, though I was enabled to satisfy myself, from the testimony of some of the female friends, that in each instance it had been stated by the unfortunate parents to have been living a short time previous to their dissolution. In what way then can we account for this, unless on the grounds of a cessation of a function of the placenta, somewhat analogous to respiration, having taken place with the mother's death, that the child immediately perished by a cause similar to suffocation. Did it perform the office of nourishing the child alone, as contended for by some, it is obvious that it might and would have survived the mother for a period of time far beyond that which I have mentioned, and must, therefore, have been found living on opening the womb ; but the contrary being the case, appears to me an indisputable proof of its performing the other part also here spoken of, or some one of the same nature.

Doctor Ireland, of this city, informs me, that on one occasion he was called to see a woman, who had died suddenly from a blow she had received from her husband ; the woman was pregnant, and evidently near her time. Dr. Ireland, who arrived almost immediately after she expired, at once perceived she was perfectly dead, but informed the bystanders that her child still lived and could be saved, but that for that purpose it should be taken from the parent with as little delay as pos-

sible. Consent being given, the doctor proceeded to the operation, and removed the child alive. It is now twenty-three years since the circumstance occurred, and the individual (a man) so saved by Dr. Ireland's promptitude on the occasion, is still living. The case deserves to be recorded for many reasons, but it is particularly applicable to the present matter, as it shows how perfectly independent the life of the unborn child, at or near the full time, is of the mother, except through the placenta, so that the death of the children in the two cases I have just adverted to, did not result in consequence of that of the mother, but of the time that had elapsed since the placenta had ceased to perform its duty. It also appears, that although the child, in Dr. Ireland's case, was exposed, and, therefore, capable of breathing in about eight or ten minutes after the mother's circulation had ceased, when the office of the placenta had been extinguished, that it was, however, then sinking, and required some exertions to resuscitate it.

“ From the general principles of physiology, as well as from experiments on the chick *in ovo*, and from the fatal effects which instantly follow compression of the cord, whilst the child is *in utero*, it is allowable to infer, that the placenta serves to produce a change in the blood of the fœtus, analogous to that which the blood of the adult undergoes in the lungs ; and from considering that the fœtus itself cannot create materials for its own growth and support, we may further infer, that the placenta is the source of nutrition also.”*

“ If interruption to the return of the foetal blood from the placental circulation occur from any cause, the life of the fœtus is as certainly destroyed as if the free passage of the air into the lungs were prevented under breathing life.”†

“ As to the placenta it possibly serves both to communicate the vivifying principle which breathing life supplies after birth, and to furnish nourishment to the embryo, though the proofs in

* Principles of Midwifery, by John Burns, p. 181.

† Practical Observations on Midwifery, by John Ramsbotham, M.D., p. 25.

favour of the former of those are much more convincing than those in support of the latter.”*

I feel it necessary to enter somewhat at large into the establishing of this point respecting the placenta, because it is important to the validity of what I shall advance in support of those doubts I have stated, and because a contrary opinion respecting the existence of such a function, is held by some of the French writers on midwifery, whose works are in much estimation, and to which I shall have occasion to advert in the course of this paper. At all events, however disputed the matter may be, one thing is certain, that an intercourse is absolutely necessary between the child and it, and of such a nature, that it must be constant and uninterrupted even for a very short period; and that this intercourse has some relation to the circulation, which is sufficient for my purpose.

Viewing then the matter in this important light, when we reflect on the admirable provision of nature, in order to the completion of the generative process throughout all its stages, the undeviating accuracy which marks the various compartments of it in every particular; an accuracy that appears to be more strictly observed, the higher these compartments ascend in the scale of importance; I think we may fairly infer, that every thing connected with so necessary a part partakes of that regularity: that every precaution is taken that its perfection or maturation shall be conducted undisturbed and free from interruption in every particular, and that its highly important functions shall suffer neither interference nor diminution up to the very moment they cease to be required. Whether, however, this could be the case, consistent with the received opinion as to its general position in the womb, the supposed mode of the expansion of this organ, and of its action in its propulsive contractions at birth; in fact, whether we have just ideas of these matters, we shall now proceed to examine.

According to this opinion, the ovum or germ of the future

* Outlines of Midwifery, by James Hamilton, M.D., p. 25.

being, shortly after its reception into the uterus, becomes engrafted on the mother by means of the placenta, which is then attached to the former, at or near its superior extremity or fundus ; as the child increases, the capacity of this organ likewise increases ; it enlarges, therefore, but this enlargement is supposed to take place equally in all directions, rising up and expanding generally, the neck and lower part being the last to undergo the process ; so that what was the fundus at the first formation and attaching thereof, is so at all periods of gestation, changed only by the expansion or enlargement it has undergone, the placenta which was originally implanted thereon being throughout the entire process of pregnancy, as well at as during parturition, at the fundus.

That such is the present received opinion upon this subject I believe is the case, although the matter is not perhaps distinctly stated to be so in the generality of works that treat upon midwifery ; but, nevertheless, that it is supposed to happen thus, is conclusive from this, that it is known to be affixed at or near the fundus on its first formation, and is thought to continue in that position throughout pregnancy afterwards.

When parturition, or labour, as it is termed, sets in, the womb, which up to this period had been passive, now takes on an active condition : periodic contractions ensue, and finally, by the diminution of the capacity of the organ which they effect, the expulsion of the child takes place, the secundines are detached and ultimately expelled. In the onset of this last state a double action goes on, the aperture or mouth of the uterus becomes relaxed, opened and fully dilated by these contractions, in order to permit of the transmission of the child ; but it is to those whereby the heretofore expanded organ is diminished that I wish to direct attention. I have already stated that these, by contracting the general capacity, exclude the child, and finally the placenta, in order to which the superficies must be actually lessened ; that is, supposing its increase to have taken place by expansion, or, what is the general opinion at present, a tempo-

rary augmentation of parts, a corresponding shrinking in, as it were, of these parts, must follow, in order to produce the diminution of its capacity. Now the supposition in this respect I believe is, that these contractions commence at the fundus, and progress from thence in all directions, anterior as well as posterior, towards the inferior portion, approximating the superior to the lower part, while at the same time they lessen the superficies of each part in succession, according as they progress, until at length it is diminished to the necessary degree.

According to this statement, then, we find that these three points—the position of the placenta in the uterus, the dilatation or expansion of the latter during pregnancy, and its consequent contractions at birth, are thus supposed to occur: first, that the placenta, on its formation, is attached to or near to the fundus of the organ, and continues in that position throughout pregnancy, with occasional deviations, all of which, except those at the neck and mouth, are compatible with safety, indeed immaterial; secondly, that the expansion of the womb is produced by a general increase of its parietes yielding in all directions, the neck and mouth being the last to expand; and thirdly, that its contractions proceed in a corresponding but retrograde order, the fundus first contracting, and afterwards the other parts in all directions, the placenta in each of the two latter continuing at the fundus throughout the entire process.

With regard to the first of these, or the original position of the placenta at its formation, there can be no doubt of the correctness of this opinion, it is a matter so well authenticated by observation and physiological proofs, that it must be at once acceded to; but with respect to the remainder, there are I consider strong grounds to question their correctness, and I shall endeavour to state my reasons for thinking so.

First then as to the position of the placenta at the fundus throughout pregnancy; as regards this, I have to observe in the commencement, that the womb expands to a much greater extent here than in any other place. The Fallopian tubes in

the unimpregnated condition, are situated at the superior part of it, at the cornua as they are termed, whereas in the fully gravid state, they are nearly one-half down upon the uterine tumour; the superficial distance between them, therefore, anteriorly and posteriorly, as well as superiorly, must be the measurement of the expansion it undergoes in this position, and is certainly there much greater than elsewhere. Now if the placenta be situated where such expansion goes on, a change of surface must to a greater or less extent after some time be constantly occurring between the two, for it is impossible their growth can go on equally together, as alluded to presently. The effect of this is obvious, according as the uterus expands or increases in superficies, the connexion between it and the placenta must be disturbed and probably to some extent torn, endangering the well known consequences of partial detachment, but at all events interfering very much with the office of the latter. When the placenta is first formed, and during the first periods of its increase, such an inconvenience might not probably arise, but after its growth has gone on to some extent, either of these events will I consider be unavoidable; the expanding uterine surface, if it do not in some degree part from the fixed placental one, must at least so disturb, and thereby interrupt the functions of the latter, as materially to influence the full and efficient discharge of its duty.

The only arrangement of parts whereby we can conceive these misfortunes could be avoided, is on the supposition that the growth of the uterine and that of the placental surface keeps pace together, as stated by Velpeau; no doubt in this respect the increase of the womb takes place to accommodate that of the child, and as the latter grows, the demand for sustenance on the placenta, (if it perform such an office,) must also increase, and so give rise to the necessity of a corresponding enlargement in it; but in order to the completion of Velpeau's theory, so as not to endanger what I have stated, the increase of each should exactly correspond, or nearly so, whereas any

person who has seen the gravid womb at its full time, will at once perceive an extension of the surface at its fundus, so far beyond that of the placenta, as to demonstrate the impossibility of such an arrangement as to their mutual enlargement going on between them at that particular part, as this implies—admitting, however, that during gestation there should be a perfect freedom from untoward consequences, when the contractions of labour set in, I cannot see how they will not occur, and in the most decided manner.

These contractions, as I have already mentioned, are supposed to commence at its fundus, and from thence progress downwards, lessening the surface they act upon as they proceed, and increasing in power with each succeeding pain; the first series of them, therefore, must, by contracting that part to which the placenta is supposed to be affixed, in the first instance lessen or constrict the uterine vessels upon which its utility chiefly depends, and thereby impede or interfere with its office to a certain extent, and the contractions going on increasing, they must ultimately extinguish its function, if not detach it altogether, and that very early in the process of the parturition. Should the contractions of a part in labour be at all commensurate with its expansion during gestation, which is highly probable, we may readily perceive from what has been remarked as to the extent of this latter at the fundus, how decidedly this must happen at that particular part of the womb. If birth took place with any thing like quickness after the contractions had gone on to the extent sufficient to produce these effects, and which I contend for they must very early in the labour, then such objections might not lie against this theory as regards these contractions; but the reverse is what happens, whole hours sometimes elapsing before the process is subsequently completed;—let any person form an opinion as to what the state of the placenta must be with respect to the impeding of its office, or even of its attachment to the uterus, when the capacity of the organ is lessened to the extent it must be when the head has

fully entered the pelvis in a labour, and then judge what the effect such a state of things must have upon the life of the child where there is delay, or even in the most favourable and expeditious cases, that is, if the opinion of Hamilton, Ramsbotham, Burns, and others be correct respecting the uses of the placenta, I mean the time that will elapse before the birth is completed, or the first act of respiration can possibly be performed, and I think he will be disposed to believe that the child can scarcely in any instance survive.

That such must necessarily be the effect of the contractions upon the placenta—extinguishing its functions early in labour, and depriving the child of the benefit it derives therefrom, is obvious, if these contractions be performed in the way I mention, and the placenta be at the fundus of the womb: the statement is by no means overdrawn, or for the purpose of answering particular views, nor is it one for the first time made; we find the same thing put forward by some of the French authors, and which I shall beg to set out here.

“ According as the cavity of this viscus (the womb) diminishes, the vessels with which it is connected contract, and become tortuous, such as they were before pregnancy, and experience a compression not only the more strong, according as its contractions upon the body it contains become more powerful, and that this body resists it the more, but also as the womb itself approaches nearer to its natural state.

“ The blood now courses through the arteries with more difficulty, and delays longer in the sinuses; these sinuses receive a smaller quantity in a given time than what they did before, and transmit less to the parts above spoken of,—its expanded tissues.

“ The blood traverses the uterine arteries with such difficulty, when the labour becomes strong and continued, (some time after the discharge of the waters,) that all communication seems to be interrupted between these vessels and the sinuses in their neighbourhood, and the sinuses themselves and those of

the placenta, so much so, that the child can no longer be considered to derive its existence from the blood coming from the mother, and that little need be feared from hæmorrhage, were the placenta detached and loose in the womb, and if any apprehensions existed before respecting it, it is at this moment arrested: the shrinking in and contraction of the womb upon itself after the expulsion of the child, and still more of the placenta, produces the same phenomena.”*

Gardien follows in the same strain, but, as it appears to me, in language more decided.

“The better to understand these phenomena,” (the changes in the womb,) “it is important to consider them in the different stages of labour, because the effects which result to the circulation differ somewhat in each period thereof.

“At the beginning of labour the blood first experiences some difficulty in traversing the vessels, but still it passes through the arteries into the different parts that are in the immediate contiguity of the womb, though slower, and in less quantity; when the waters are discharged, and the womb contracts with force, the vessels can no longer deposit the blood in the cells of the placenta but with the greatest difficulty, because they are too strongly compressed; the succeeding contraction being much more energetic, all communication is intercepted between the vessels of the womb and those of the placenta—there no longer exists a communication between the mother and her child; hæmorrhage can no longer occur if even the placenta were detached, and if it existed before it is arrested.

“The circulation, however, still continues between the child and the placenta; but when the womb contracts again this ceases altogether, because the placenta is too forcibly compressed by the body of the child; the blood, however, still passes into the umbilical arteries, and from thence into the vein by means of the communication which exists between them;

* *L'Art des Accouchement*, par J. L. Baudelocque, pp. 98, 99.

but when the contractions become more strong, the cord is compressed against the border or brim of the pelvis, and the blood then, which had before been transmitted to the child without going through the placenta, in consequence of the communication between the umbilical artery and vein, can no longer take that course, and the brain would become engorged if the circulation in the natural way were not soon established.”*

The same sentiments are delivered by Capuron, (pp. 151, 152,) and almost in the same terms, but I think it unnecessary to repeat them here.

This view of the matter, however, is perfectly compatible with the safety of the child, according to the opinion generally entertained by French writers with respect to the office the placenta fills in pregnancy; the general supposition in this respect being, that it serves only the purpose of conveying nutriment to the child from the blood of the mother, which is thought to be first elaborated in the placenta, and then taken up by the umbilical vein, and conveyed into the infantile system, the part it is supposed to perform in the way of regenerating the infant's blood, by a process analogous to what takes place in the lungs in breathing life, being considered doubtful, and denied altogether by some.

“The property of purifying the blood, of rendering it thinner, lighter, more charged with oxygen, in fact more fitted for the nutrition of the child, is an office I only attribute to it, (the placenta,) agreeable to the opinion of some modern physiologists, and particularly some of the ancient ones, who were so persuaded of this, that they called this intermediate body *hepar uterinam*, uterine liver, from analogy to the functions of the liver, which they regarded as the principal organ of sanguification.”†

With this opinion of the nature of its office, these authors may readily suppose a suppression of it for such a length of

* *Traité Complet d'Accouchement*, par M. Gardien, pp. 174, 175.

† Maygrier, p. 140.

time, to be perfectly compatible with the safety of the child ; but if it perform one similar to that which I have urged, and which is, I believe, very generally admitted, an interruption of but a very few minutes indeed, must be fatal to it, much less one to the extent implied in the above abstracts ; nevertheless if the contractions of labour take place in the manner now supposed, all parts acting together, we can scarcely deny the correctness of the statements contained in these abstracts, the one must in fact be a consequent of the other ; the inference, therefore, I think, is, that we are misinformed some way respecting the matter, that the nature of the contractions of the womb is not well understood, and that something yet remains to be known generally on the subject.

With such opinions very much impressed on my mind, and which became the more strengthened the more I reflected on them, my attention was particularly directed to these points in a dissection that occurred at the Coombe ; the subject of it, who came into the hospital on experiencing the first signs of approaching labour, having expired a short time after her admission. In this instance, where we had an opportunity of conducting the examination with the advantage of observing all the parts in their natural situation, the placenta was found to be affixed—*low down at the posterior part of the womb.*

Since that case three others of a similar nature have occurred to us likewise at the Coombe, the women in two of them having expired at the expiration of the full period of gestation, but immediately preceding or at the beginning of labour, and in the third at the eighth month : and in all of them the same observation was made,—*the placenta was at the posterior part of the womb.*

In addition to these four cases, I call to my recollection one which occurred in an indoor patient, where the afterbirth was retained for an unusual length of time after the expulsion of the child. In this instance the womb appeared to have relaxed completely, fortunately before any detachment of the placenta had

taken place ; and on introducing my hand therein, in order to its removal, I found it not at the fundus,—*but low down at the posterior part.*

Some short time since, also, we had an examination of the body of a woman who died three days after her delivery ; and on inspecting the womb it was found somewhat relaxed, the position where the placenta had been attached perfectly discernible, coated over by something resembling placental matter, which appeared to seal up the vessels, and so prevent the passing off of the lochia, and in this case also,—*the posterior part of the womb had been the place of its attachment.*

These dissections were carefully conducted by my colleague, Mr. Power, and myself, and in the presence of the pupils of the hospital.

It may not be unnecessary here to remark, that in examining the pregnant womb in the dead subject, with a view to ascertain the relative position of its parietes or sides, allowance should be made for the contractions this organ is known to undergo even after death, and with considerable force, and which will materially alter those positions, particularly if the waters have been discharged. Baudelocque observes this fact, and states in a note, that several authors attest, that women have been known absolutely to undergo spontaneous delivery even after death. Maygrier also mentions a case in which a woman underwent it forty-eight hours subsequent to dissolution.

The invariable position the placenta was observed to have in all these cases, at the posterior surface of the womb, naturally induced me to a further investigation of the matter in the several other ways the inquiry could be prosecuted in ; and the testimony of all these are, I find, in support of this position, indeed they render the matter conclusive.

In the Museum of the Royal College of Surgeons, there are two preparations, Nos. 326 and 343, in which the child with its investing membranes are beautifully displayed *in utero* ; and in both these the placenta is affixed at the posterior surface.

We have two preparations also at the Coombe Hospital, one a ruptured uterus with a portion of the placenta remaining, and in this likewise the posterior part is the place of its attachment. The other is a preparation of a gravid uterus near the full time, with its contents *in situ*; and here also the same observation is to be made, the placenta is at the posterior surface.

In Hunter's plates on the gravid uterus, it will also be found, that in the majority of them, where the position of the placenta is delineated, that the posterior part is that to which it had been affixed.

So far with respect to the evidence on the subject which is to be derived from the foregoing description of testimony; we shall now proceed to the consideration of others, which though somewhat inferential, are, I think, equally striking and satisfactory; and the more convincing, inasmuch as they admit of a more extended use in the inquiry.

It has lately been ascertained, that when the ear is applied to the stethoscope placed on the person of a pregnant woman towards the close of gestation, that the placenta may be detected by a particular sound transmitted from it through the instrument, and denominated in consequence of its peculiar resonance, though in my opinion erroneously, the placental murmur. Now, I would ask, why does it happen that if the stethoscope be applied at the fundus of the womb, where the placenta is supposed to be affixed, that this murmur is *never* heard there? the reason is obvious, the placenta is *never* there: when I say *never*, I of course mean in natural and healthy pregnancies, such as, I believe, we are chiefly in the habit of observing in this city; and yet it would certainly be reasonable to imagine, that if such were its position, in that particular situation would this indicative sound of its presence be infallibly detected, and that there should the stethoscope be placed, in order to discover it; at all events, I cannot conceive how it could happen that with the stethoscope placed *upon* the placenta, and which it must be if the fundus be its position, that not the

slightest trace of its sound should be discernible, yet such is the invariable fact. If, however, the stethoscope be applied at the side and lower part of the womb, somewhat above the groin, in one of the iliac fossæ, its sound is always at once here detected; it is almost useless to say why, because here the placenta is always in its immediate vicinity, nay, immediately under it.

This sound is heard generally in one groin only, and is silent in the other, the reason of which admits, I think, of a very easy explanation. When the ovum descends into the uterus from its ovarium, it is shortly after implanted thereon, as already mentioned, and it is, of course, obvious that this attaching of it must take place nearer to that Fallopian tube which it traversed than the other; now observation shows, that in whatever manner the uterus enlarges during pregnancy, an expansion of it takes place between the Fallopian tubes, at the posterior part of the organ, (such I observed invariably to be the case in the four dissections above stated,) the necessary consequence of this then must be, that in the separation, as it were, of the two tubes from the middle line, the one which gives transmission to the ovum must, to a certain extent, carry it and its placenta along with it, and thus give rise to the fact of this resonance being generally audible in one iliac fossa, and not in the other.

I cannot agree with Velpeau, in supposing that the seat of this sound may be in the arteries connected with the pelvis; that after the uterine tumour has increased to a particular size, it presses on these arteries to a certain extent, and by thus impeding the passage of the blood through them, so gives rise to it. If such were its true explanation, it is obvious, that inasmuch as this pressure must be exercised equally on all parts, that the arteries of one side of the pelvis would be as much affected by it as those on the other, and that, therefore, it would always be heard equally on both. I am aware that this latter sometimes is the case, but it is very rarely so, and when it is, it is very indistinct in one, while it is full and sono-

rous on the other. It is also immaterial whether that side in which it is to be found, be above or below the uterine tumour in the recumbent posture of the patient, the sound either as to fulness or locality is in no way affected by it, and which evidently would be otherwise were this doctrine correct.

The truth most probably is, that the seat of it lies in that portion of the uterus to which the placenta is attached ; that it is in the structures of the mother, is evident from the fact of its pulsations corresponding with her circulation, with which, therefore, it must be connected, and the part of the womb where the placenta is affixed, being at these periods more freely supplied with blood than the other parts, gives every reason to suppose that to this particular place it is to be referred ; it may therefore be questionable, whether, correctly speaking, it should be designated the *placental* murmur.

In fact, the diameter of the full grown placenta is about five and a half to six inches ; that of the womb, at the lower part of its body, towards the close of gestation, where the placenta is about fixed, from eight to nine inches ; making allowance then for the deviation from the centre, which must arise in consequence of what I have stated of the expansion between the Fallopian tubes, the border of the mass, or rather of its attachment, must lap somewhat round the womb at that side it is originally implanted on, and thus give rise to the fact that it is generally heard in one groin and not in the other.

Whilst prosecuting this inquiry at the Coombe Hospital, (the position of the placenta in the womb,) a very ingenious plan of investigating it was suggested to me by Mr. Reardon, one of our students there, which in my mind puts the matter altogether beyond doubt ; I find the same thing stated by Maygrier, though not so fully to my purpose as we have employed it at the hospital, namely, by inspecting the placenta with the membranes attached, and undisturbed, after their delivery.

The child before birth is enveloped in a double cyst of membranes proper to itself, independent of one formed by the

womb at conception. These membranes, though unattached to the uterus, are firmly united to the placenta and cord, and do not come away in labour except with the latter. It is obvious then, that in order to birth, the child must first perforate the cyst, and thus form an aperture therein ; now if care be taken in the delivery of these parts, that the membranes shall be preserved unbroken, except at that place through which the child escapes, the information we require may be obtained with the utmost correctness ; for example, if the borders of the aperture be equidistant from the placenta in all directions, then this latter must have been exactly opposite, and consequently at the fundus, for the aperture is obviously at its mouth ; but if, on the contrary, the distance be considerable in one direction, while it is but a very little removed from the placenta in the other, then it is evident that the fundus was not the position it occupied, but somewhere not far from the mouth of the womb ; in fact, by inflating the membranes so preserved, we have an exact representation of how they and the placenta were situated *in utero*.

This is the manner Maygrier states the matter, but in this way it would only serve to show how far from the fundus the placenta was situated, without pointing out its exact position, and therefore would be insufficient for my purpose, because that may be in any part, anteriorly, laterally, or otherwise ; but by introducing the hand into the opening, we get it into a pouch formed by the membranes, the superior part of which evidently corresponded with the fundus, and then by running the fingers along their inside, opposite to where they are attached to the placenta, we are at once enabled to perceive, by their peculiar appearance there, that at that part they lined the anterior, distended, and concave portion of the womb, and thus consequently fix the back part of it to be the place where the placenta was attached.

In order to make this examination, it is necessary to be aware how the placenta comes down, and is born ; it precedes

the membranes, turns them inside out, passing through the opening in them, so that to put the parts in their proper position, and which is required for our purpose, it will be necessary cautiously to repass it and the cord through the aperture, taking care that they are not further broken.

From Maygrier's remarks, it is obvious he was not aware of the information this plan is capable of giving, at least he does not seem to have availed himself of it, for he observes that the same information may be gained by introducing the hand into the womb when the child is born, and before the placenta is detached ; I shall in a further part of the inquiry, however, show, that this is quite a deception, and must lead us altogether astray, if we depend upon it for ascertaining where the placenta was situate during pregnancy ; according to him the placenta more frequently occupies the middle and back part, and but rarely the fundus.

I have in numberless instances tested the matter by the above means, observing the precautions I have stated, and have invariably found the posterior part of the womb to have been the place of its attachment, and in the great majority of them low down. I have had all the placentas for a length of time past preserved for my inspection at the Coombe, and had those in the extern cases reported to me, and they all, without an exception, bear testimony to this fact.* In a word, so extensive is the mass of evidence afforded me in this way, which I think quite as demonstrative as even *post mortem* examination, that I look upon the question as settled ; there can be no doubt of it ; every practitioner can satisfy himself of its correctness, that is, in natural cases, where there is no hæmorrhage.

Since our attention has been directed to this mode of inquiry at the Coombe, it has led us into very particular examinations of the placentas, and we find that in those cases where

* We deliver from about 140 to 150 women per month, at and from this hospital, between the intern and extern cases.

the delivery has been affected without hæmorrhage, they are situated low down on the posterior part, and would seem to be not more than an inch and a half from the most depending part of the cyst, and sometimes less; but when the labours have been attended with hæmorrhage, this test showed them to have been somewhat high up, where they must have come within the range of early uterine contraction, and so have been partially detached before those contractions were sufficient, and the parts themselves so circumstanced as to be secure from bleeding. I have to observe, however, that it is but lately our attention has been directed to the investigation of this particular point, and the number of cases illustrative of the matter, though not by any means few, are not, however, sufficient, to speak of it in more decided terms than what I do, but so far as we have had opportunities, they certainly fully supported such an opinion. I have never seen a placenta with its membranes that indicated its being affixed at the fundus, in the gravid state, or even very near it; and judging from the disposition to hæmorrhage which was observable when they were high up in the uterus, I should say an attachment at the fundus must be attended with the consequences I have already stated, when reasoning on the matter in the beginning of this paper. Within a very few days since, Mr. Bannin, our resident pupil, informed me of a case complicated with very considerable hæmorrhage, which occurred in an out patient; and on inspecting the secundines, he found the placenta was even higher up towards the fundus than what we have generally observed it to be in births attended with bleeding, in the less severe forms we generally witness it in.

In Denman's *Treatise on Midwifery*, when speaking of the Cæsarean section, he states a case in which that operation was performed in consequence of deformity of the mother, and mentions, that on cutting open the womb to extract the child, the placenta was found at the fundus, and towards the anterior part. This fact certainly would at first seem to make against my views, as here given; but on reflection, I think it admits of

explanation, at least it may be shewn that the case cannot be authority as to gestation in any of its departments.

The process of generation, in the human race, appears to be one of the most delicate of all the ordinances of nature ; the operation of several separate and distinct systems are concerned in its execution, all of which individually and collectively are so necessary thereto, and linked so together, that in order to the perfection it usually takes place in, they must all be in operation healthily and free from derangement, otherwise error in some respect may ensue ; for example, when menstruation is faulty, particularly when in excess, conception frequently does not take place, though every other requisite may be present, and in a proper state, nor will the womb become fruitful till its healthiness in this respect be restored. Again, it is an observation made by many, that those labours denominated preternatural labours, where the part of the child that should first be born (the head) is not so, but other parts offer for that purpose, or present, as it is termed, as the arm, foot, breech, &c. ; it is observed, I say, that these occur most frequently where disfigurement in some shape is present in the bony structure of the mother, in so trifling a degree does faultiness in any, apparently the most immaterial point, appear capable of affecting the harmony that in a natural state of things is found to prevail. We are not, however, as I think, to confine this disposition to err in the usual arrangement, which may take place in such cases, to the presentation alone ; we may, I consider, go further, and suppose it may also extend to, and influence other parts.

Now, in Denman's case, it will be recollected that the bony structure of the mother was distorted to such an extent, that even were the child broken up in the womb, it could not obtain transmission through it, and therefore we are not to be surprised to find the placenta in the position it is reported to be in ; at all events, any thing met with in such a case, cannot be looked on as authority, or a standard or general rule, in generation. I have been also informed by Mr. Porter, the President

of the College of Surgeons, that in a case in which he was called on to perform this operation, the placenta was likewise situated in the very same situation mentioned in Denman's case : in that of Mr. Porter, the cause for the operation lay in a large tumour that completely blocked up the pelvis ; it was, however, sufficient to vitiate the usual arrangements of nature, and consequently the same remarks will apply to it as to the other just stated.

It is probable the two facts already mentioned, namely, the admitted one of the placenta, on its first formation, being affixed at or near the fundus ; and, also, that when the hand is introduced into the womb for its removal, where it is retained for an unusual time, its being found likewise in the same position, for it is not then completely upon the fundus, have led to the opinion now generally entertained, of its occupying that position throughout the entire of pregnancy.

By these, however, we have only evidence as to its situation at its formation, and also when it is useless, and about to be removed from the system, but how it is circumstanced in this respect in the middle periods, or that it may be then differently situated, appears to have escaped notice ; the matter certainly is involved more or less in obscurity, and difficult of investigation, and it is probable, therefore, physiologists have given such vague accounts of it, as that it is and may be affixed at any part indifferently, the mouth and neck excepted.

I think, however, I have adduced proof sufficient to satisfy most minds, that at the close of pregnancy the placenta is low down at the posterior part of the womb, and such being the case, we have it apparently changing its position thereon in different stages of gestation ; first, at its formation it is at the fundus ; afterwards it is situated low down on the posterior part ; and finally, when the child is expelled, it is again at the fundus. The consideration of this, however, or how such a change of position occurs, leads us to that of the second part of my subject, namely, the manner in which the expansion or enlarge-

ment goes on during gestation, and will point out to us how this expansion, and also the subsequent contractions of labour, may take place without involving the consequences I have spoken of above.

I cannot for a moment suppose that the placenta absolutely alters its position in the womb, leaving the part it was first implanted on, and sinking on the posterior wall ; if such were the case, how are we to dispose of the original connecting matter between them, be that vascular or otherwise ? Let the change of posture be ever so gradual, would it not have the effect of constantly disturbing the circulation, and thereby the functions of this highly important part, and does not the supposition, therefore involve an absurdity ? No ; on whatever part it is first affixed, on that identical part does it remain throughout the entire of gestation afterwards ; and this apparent alteration of position not only must be the consequence of a change taking place during the subsequent stages of that process, between the relative position of the parts of the womb, but it likewise proves that such a change of the relative position of the parts does absolutely occur.

There is but one way then of accounting for it, namely, that the fundus where, or near to where the placenta is first attached, together with the parts posterior to it, undergo little or no alteration or change of surface during gestation, beyond what is sufficient to accommodate the increase of the placenta, and that the expansion principally goes on at the anterior part. The womb, however, in its increase undergoes it chiefly in its altitude, the supply for which must, therefore, come principally from the anterior wall ; now the effect of this must be, that while the posterior part, and so much of the fundus as is occupied by the placenta, remains nearly stationary, except to the extent just mentioned, (a thing very much in accordance with reason, seeing how necessary it is that the placenta should be as little disturbed as possible,) that the anterior portion expands and rises up, each superior part of it arriving at and occupying the fundus,

and then turning over to form a portion of the posterior parietes, until the expansion has gone to the required extent.

If the premises I have assumed be correct and admitted—firstly, that the placenta is formed and attached at the fundus, or somewhere near it; secondly, that it remains throughout pregnancy on the same identical portion of the womb it was first attached to; and lastly, that at the close of gestation it is low down on the posterior part of it, I cannot see in what other manner we can account for this change of position. The first of these will be, I think, fully admitted. The second is the only rational supposition; and the third, I consider, I have proved. Now if this be the case, we have in fact the virgin or original fundus, where, or near to where, the placenta was formed, at the posterior part at the full time; what then forms the portion of this posterior part superior to it at this time, namely, all that, now above the placenta or Fallopian tubes? That which lay anterior to it when it was the fundus, that is the superior portion of the anterior wall, which, to arrive at its latter position, must have occupied and passed over the fundus in the way I have stated.

That the womb expands posteriorly also must be admitted; but the expansion here is more in a lateral direction, the space between the Fallopian tubes being considerably increased: it is at the same time somewhat elongated, though compared with other parts in this respect, it is but triflingly so. Now, if its increase took place in the way it is usually thought to do, by a rising up and general expansion of its parietes, the Fallopian tubes would still occupy their original position at the fundus, divaricated no doubt considerably, whereas, as I have before remarked, they are found somewhat more than one-third downwards on the tumid womb. In two of my dissections, I found them on the posterior aspect, in the other two they were at the sides.

Such evidence as the relative thickness of the womb affords in these different positions in the fully pregnant state, is like-

wise favourable to this view of the matter. In all my cases it was thickest posteriorly where the placenta was attached, the same remark is made by Gardien, and thinnest at the fundus and anterior part. The reverse of this is the case after delivery, a section of the organ showing the thickest part then to be anteriorly, and towards the inferior part, at least so it was in a few cases of this kind which I had an opportunity of examining.

I have stated that at its formation the placenta is affixed at the fundus or close to it; but it may probably be said, that it is not exactly so placed; still it must be very nearly so, near the Fallopian tubes. Admitting, however, that at its first formation it should be somewhat removed from the fundus, even inferior to it, and making every allowance for this, without the supposition I have stated, it is impossible it could afterwards attain so low a position as the evidence I have mentioned points out to us.

Having taken this view of the manner in which the womb enlarges during gestation, so different, I believe, from that heretofore supposed, (at least I am not aware of such a one being at any time before put forward,) it naturally leads us to a theory of its contractions, during parturition, likewise altogether different from the present opinion respecting them, which fully explains how these contractions can and do go on up to the very completion of the birth, preserving the functions of the placenta utterly unimpaired to the last; and in opposition to the doctrine of Baudelocque, Gardien, and Capuron, already stated, which holds that the placenta and cord are so compressed by these contractions, as also the uterine vessels, in some manner in communion with them, that their circulation is entirely suspended for some hours, I think I may say, during labour, goes far in still upholding the respiratory office of that appendage, or something similar to it.

In whatever way the womb enlarges during pregnancy, I take it to be a rule, that the very reverse is the order of its con-

traction ; it expands for the accommodation of its increasing contents, and labour, as it is termed, is a retrograde movement, for the purpose of regaining its normal condition, and so excluding the child.

I look upon the placenta as one of the most important parts in the entire of this piece of mechanism. The proofs of the necessity for its functions to the last are, I think, indisputable, it is needless here again to repeat them ; upon this, the consummation of the work depends—the birth of a living being.

Now according to the statement I have made, as to the expanding of the gestating womb, the posterior portion of it, the depositary of the placenta, except so far as is necessary to accommodate the growing surface of the latter, remains nearly quiescent during that process, a most important matter, the supply for this purpose being taken from the anterior wall chiefly. The active part in labour, therefore, or that where the operation of these contractions is carried on, must very principally be confined to this department, if the rule I have laid down be correct, leaving the back, where the placenta is still lodged in an undisturbed state.

The contractions then going on chiefly on the anterior part, they necessarily must first draw down the fundus, which now appears on the anterior wall, and these contractions continuing and increasing in energy as they proceed, each portion of it which passed over the fundus, during the expansive process, to the posterior position, now repasses under the influence of these contractions, regaining its original position, thus diminishing its cavity, and ultimately delivering its contents.

I think I have laid strong grounds in support of the expansion of the organ taking place in the way I have stated, and if the contractions be in the same order only reversed, it follows that this or something like it is the manner delivery is accomplished ; if otherwise, we must either not admit the first, which, I think, would be difficult, or deny the contractions to follow the order of the expansion.

Although the investigation of this part of my subject is certainly involved in very great obscurity, we are not, however, without very striking facts in support of this opinion.

If the contractions took place in the way generally imagined, on all parts, superiorly, posteriorly, and anteriorly, commencing and progressing in the way supposed, the effect on the placenta and cord, together with that on the uterine vessels, must indisputably be such as is stated by the French authors just adverted to, their circulation must first be impeded and then stopped, and that early in the labour ; there can be no doubt but that such a result must inevitably follow according to the common rules of physics. However, that there can be no interruption to the placental circulation, until a very short time previous to the child being born, I think is conclusive from the remarks already made, and that the uterine circulation connected with the placenta does not suffer any diminution up to that event, we may satisfy ourselves of by stethoscopic investigation. I have in numberless instances (in every case where I tried it) detected this circulation in full operation by means of that instrument, when the child's head was even lying on the perineum, just before birth, and when consequently the contractions were such as must have completely constricted the vessels by which it is carried on, and have, therefore, extinguished this peculiar sound which depended upon them, if these contractions took place in the way they are thought to do ; I have even detected this sound, (the placental murmur,) when the womb was slightly under the influence of the ergot of rye, and at this same stage of the labour.

That this sound results from the mother's circulation, in whatever part it is produced, is evident, as I have already observed, from its being synchronous or simultaneous with the pulsations of the mother's heart, and, I think from the fact of its continuation up to the above period, there are strong inferential grounds for concluding that the placenta itself is then also in full operation, independent of the proofs just alluded to, for

otherwise why would the increased uterine circulation here be continued, when it is evidently only for the purpose of ministering to that of the placenta, or to what use would it tend if the placenta were so compressed as to be incapable of receiving and being benefited by its influence.

From these facts, then, I conclude that the statements of these authors are erroneous, and consequently that it is incorrect to imagine the contractions of the womb go on as at present supposed, upon the authority of which these statements are based.

In what possible way, however, can we account for the uterine contractions taking place up to almost the very last act of labour without the circulations being impeded, which are so essential to the life of the child: clearly on the grounds alone, that the part where the placenta is attached, the back of the womb, not only is not submitted to these contractions during the labour until after the birth of the child, but is likewise by some means preserved from the effects which the contractions in other parts would have on it (compression) without some such means being in operation.

Now this is precisely what would result from the manner I have contended that these contractions proceed; they are all on the front of the womb, pulling down the fundus towards the neck in that position, the foetal body being the fulcrum on which they act, so that the stronger they do act, so far from constringing the uterine vessels, and thus cutting off their communication with the placenta, as they unquestionably must do according to the present doctrine, they throw the part to which it is attached more upon the tension, and thereby instead of impeding, facilitate the circulation in these parts, at a time when certainly the firmly compressed child stands the most in need of its aid.

I think it necessary to observe, that in examining with the stethoscope for the purpose just stated, it will be found that the placental murmur, or soufflet, ceases *during* a pain or contrac-

tion ; this, however, is not altogether in consequence of compression of the uterine vessels, that go to supply the placenta, or of this latter itself, from the body of the child being forced against it, as a little attention will shew. Take, for example, any one contraction after the second stage of labour has somewhat progressed, and during its continuance the soufflet ceases ; however, if we again examine when the labour has so advanced that even the head is upon the perineum, although during a pain it is also then suppressed, still in the absence of one, a distinct, full sound, is audible, in some instances I have thought even more so than in the beginning of the stage. Now at this latter period, compression on these parts from permanent contraction must be much greater, even when a pain is absent, than it was in the first experiment, when the pain was on, the uterus by this time having considerably lessened its capacity ; and, therefore, the fact of its being then full and audible, is a decided proof that this suppression of sound, during the contractions, is not a consequence of compression, as put forward by the above French authorities, but results from some other cause.

The fact is, in the second stage of labour, when the strong contractions are on, the pulse of the mother is always affected, suppressed, or nearly so, and it is in accordance with this, that the placental sound then ceases. The circulation of the mother, it appears, is influenced by the general excitement that pervades the system when under the effect of these vigorous contractions ; that of the uterine arteries, therefore, must experience a similar temporary suppression, and with them the placental sound, whose existence upon their action depends. I have repeatedly satisfied myself of these facts at the Coombe Hospital, and have pointed them out to our students at that establishment.

It is a very difficult matter to conduct an examination of this nature with the necessary degree of exactness, as may be conceived from the state of the patient at such a period ; in order to it, the labour should be attended to by an assistant, so that the undivided attention may be given to the observations, and on this

account it probably could only be conveniently effected in hospital practice.

There are some few facts connected with parturition which receive an easy explanation from such a mode of uterine contraction, and which, therefore, may be received in some degree as testimony in support of it. The prominent appearance which the fundus frequently assumes as the second stage advances, is readily thus accounted for : it is then generally thrust forwards, and, at least apparently, considerably beyond what it was before labour commenced. Now if the contractions operate in the way I state, on the fundus and anterior part principally, this is precisely the result that would follow ; the superior extremity of the uterus must necessarily be drawn forward by them, giving the uterine tumour this projected appearance. We have likewise an opportunity of observing here the economy of nature in effecting more than one object by the operation of the same cause ; while the placental influence is thus preserved to the last, by advancing likewise the superior extremity of the uterine tumour, the axis of the foetal body is thereby thrown into that of the entrance into the pelvis, through which it has to pass, and in its transit of that part, the force by which it is propelled is also in the same direction ; if, on the contrary, the contractions took place in all directions, and that the fundus approached the mouth of the womb, in obedience to such a mode, the uterus would thereby be drawn directly downwards, and the head of the child be propelled rather against the pubis than in the proper direction. Further, how, I would ask, does it happen, that when the child is expelled, when the uterus can be distinctly felt at the lower part of the abdomen, hard, and somewhat about the size of the child's head, that the placenta is still adherent and fixed, and generally is not detached for fifteen or twenty minutes after, and sometimes not for a considerably longer period ? There is but one solution of the question ; because the contractions which have affected all the other parts have not yet influenced the placental department, there can be

no other reason. Can we suppose, with the womb reduced to that size, if the contractions have operated generally on its superficies, that the placenta would still adhere? if we do, then all our reasoning as to the manner in which it is detached by these contractions is wrong. Is it not an invariable injunction laid down by all, not to pull at the cord, nor endeavour to tear away in that manner the placenta, but wait even then *till it is detached*? but in so contracted a state, I am at a loss to know how it can be undetached, unless in accordance with what I contend for—the placenta uterine surface (the posterior part of the womb) has not yet undergone contractions, and, therefore, it still adheres. It cannot for a moment be said, that while the contractions were sufficient to expel the child, they were not so to detach the placenta, for if we only consider its diameter at full size, (near six inches,) and compare it with that of the uterus immediately after the expulsion of the child, such a thing will evidently be impossible, that is if the surface generally be affected by them. I have frequently observed the uterus, some time after the birth of the child, to go down, sink into the pelvis, and then rebound as it were, and reappear above the pubis in a firm contracted state; and it is probable in this instance, that it became lessened to that extent in order to the complete separation of the placenta, which having been effected, it partially relaxed so as not to interfere with or prevent the forthcoming lochial discharge, the transmission of which is indispensable for the life of the mother; for, certainly if the contractions took place to such an extent as completely to constrict the uterine vessels from whence it proceeds, and then remained permanent, a suppression of this discharge must ensue with all its fatal consequences. It is likewise probable that those sources of suffering to puerperal females, denominated after-pains, are much connected with this circumstance, an opinion which will receive some support from the fact of their being generally relieved by antispasmodic remedies.

We shall now take a brief view of the principles I have

been endeavouring here to advocate. I felt it necessary in the commencement, for reasons which must now be obvious, to establish the fact of a constant intercourse being required between the child and the placenta, considerably more inoperative than what would be were the latter only an organ for supplying nutriment to the former, and, therefore, the subject was gone into more at large than would probably be necessary under other circumstances.

First, then, I consider the placenta does not occupy the fundus of the womb during gestation, the part it is now, I believe, generally supposed to do, but that its true position there is its posterior part, probably the lower down the further the gestation is advanced; and I have stated my reasons for objecting on principle to the first of these opinions, and also the observations, anatomically and otherwise, that led me to the latter conclusion, having first determined the former opinion to be wrong. Secondly, I consider the enlargement of the womb does not take place by a general expansion of its boundaries, rising upwards and distending in all directions, while they preserve their relative position with respect to each other throughout, but that this increase occurs on the anterior wall principally, and probably so much of the fundus as is unoccupied by the placenta at its formation, leaving the posterior part, to which the mass is affixed, undisturbed during the process. For this latter opinion, I have likewise stated my reasons, and which it will be seen have been rather the result of first having determined the true placental situation *in utero*, while at the same time also, on principle, I object to the first; and lastly, viewing the grades of uterine contraction as the converse of those of its expansion, I consider these contractions to be confined chiefly to the parts engaged in the latter, while the posterior part of the womb, where the placenta is lodged, like as it is in the expansion, remains also undisturbed during the last process. I further have stated my reasons for supposing, as the contractions of the womb necessary for the expulsion of the child appear to be confined to

its superior and anterior part, that when the placenta is situated above its usual position on the lower part of the back of the uterus, it comes then within the range of these early contractions, and is thus partially detached with the consequences thereof; that, in fact, labours, complicated with hæmorrhage, may possibly be the result of such partial malposition, and I am of opinion, that future experience will prove the correctness of this statement.

I would now compare the effect which the theory at present received is calculated to have upon the process of generation generally, as regards these different points, with that which would result from the one I have here submitted, and see which of them is more in conformity with the mechanism and precision that so strikingly pervade every other department of it, with the exception, I must say, of those I object to. That the placenta is one of the most important parts in the entire of the machinery must be admitted; it is the hinge upon which all turns; the intent of conception and gestation is a living child, and all is abortive without it. If the placenta then be at the fundus of the womb, it is situated at the most expansive part, it is therefore, as well during gestation as labour, exposed to all the evils its unchangeable surface must here give rise to, and thus becomes utterly deprived of that quiet, an organ so important necessarily requires. I care not whether the expansions take place according to the present theory or that which I now propose, this is the part most subject to them, and consequently under labour to a greater range of early and decided contractions with their consequences. Again, admitting the placenta to be at the posterior surface instead of the fundus; although these evils might not there exist to the same extent, nevertheless, if the contractions of labour be general over the organ, they must to a certain extent, which is short of the perfection of nature; but besides this, compression, such as the authors alluded to have described, must necessarily happen, in my mind fatal to the child, and defeating the object intended. In a word, according to

such a theory, the placenta in every stage, from its formation to its end, is in a constant state of disturbance, must be exposed to interruption during pregnancy, and at labour, to suppression by detachment and compression long before it ceases to be required.

By that which I propose, it would appear, as if nature, aware of the importance of this appendage, and that upon its preservation undisturbed in these different stages, all the success of her work depends, provides from the outset most carefully for its security in all these different stages. After its formation, the first inroad of interruption to it must be derived from the enlargement of the womb, interfering with it each moment of gestation by the expansion of its surface ; this, however, is so contrived, that the effect of this threatened source of disturbance is to make it the very means whereby the placenta is placed beyond the pale, not only of it, but of any other danger it otherwise might be exposed to ; like the contractions of labour, which tear asunder the large uterine vessels that open upon it, when it is detached, though they bring the female to the very brink of instant destruction by the fearful hæmorrhage they expose her to, nevertheless, are the very means to which she owes her safety and her life. By the first act of expansion, the placenta is turned over to the back of the womb, where it continues uninterrupted throughout the remainder of pregnancy, amid all the activity of the organ upon which it is implanted ; the fundus now with impunity expands, the womb itself enlarges and distends, while that part of its surface upon which this highly important part is affixed, continues in the state of quiescence so essential for its security and its duty. Thus throughout gestation, during every moment of which, disturbance in the uterus is going on, the placenta is secured from it, and the danger it otherwise must be exposed to. When labour sets in, like the enlargement, the contractions are conducted on the anterior part of the womb, the then fundus likewise participating in them ; by the parts endeavouring to regain their natural position this lat-

ter descends ; the cavity of the uterus is thus lessened, while that part upon which the placenta is attached, is preserved not only in a perfect state of quiet, but in a condition whereby the circulation so important to the success of the process is rather facilitated than impeded ; at length, by this means, the child is expelled, and by that time the placenta arrives at the fundus ; it is now no longer required, it is useless, is there, then, submitted to the effect of these contractions, is detached, and thrown off from the system.

ART. XXII.—*Propositions relating to Diseases of the Stomach.* By JONATHAN OSBORNE, M.D., Vice-President of the King and Queen's College of Physicians in Ireland, Physician to Sir Patrick Dun's and Mercer's Hospitals, Member of the Royal Literary and Historical Society of Quebec, &c.

PART II.—AFFECTIONS OF THE MUCOUS MEMBRANE.

ON a former occasion, (Dublin Journal of Medical and Chemical Science, July, 1835,) I commenced a series of observations on diseases of the stomach, by a description of that disease which has been long known under various names, as dyspepsia, gastralgia, gastrodynia, acor ventriculi, heartburn, biliousness, &c. I endeavoured to show, that this disease essentially consists in irritation of the glands or crypts which secrete the sour fluid known by the name of gastric juice, and that when neglected it at length terminates in a scirrhus thickening of the pyloric extremity of the stomach, and in ulceration of the same. I now proceed to treat of affections of the mucous surface, which, although often called by the above names are totally different not only in their seat, but in their symptoms when properly defined, and above all differ in this, that they require an opposite mode of treatment. Hence has arisen the discrepancies and contradictions in the statements of both practitioners and patients, when they give an account of what they have expe-

rienced of stomach complaints. What one has found to be useful, another finds prejudicial. Some have described the stomach as a *microcosm* different in each individual, the laws of which are to be known only by the proprietor himself from his individual experience, and not from what has been experimented in the stomachs of others. The remedies extolled in the highest terms, when tried in complaints bearing the same name, are found not only to disappoint expectation, but often to have a direct contrary effect. When these facts are properly investigated, it will appear that the success described to have attended opposite remedies in cases of the stomach, is chiefly to be attributed to the opposite nature and tendencies of those two diseases, and that the obscurity in which they have been involved has been kept up by their being sometimes coexistent in the stomach of the same individual, although a correct view of them can be obtained only from the contemplation of them when existing separately. In the majority of cases they do exist separately; and it is only from numerous dissections that we are enabled to see, that when the two sets of symptoms coexist in the same individual, that then the two diseases are also coexistent.

I shall continue to use the form of propositions or aphorisms which the reader studious of brevity will find convenient, as by reading them he may learn the conclusions to which I have at present arrived, without troubling himself with the commentary. If those propositions appear incomplete in their application as well as abrupt in expression, I have to plead in extenuation, that my object has been to adhere strictly and exclusively to what I have myself observed, and that I have often declined to insert words which the reader may think should have been inserted, and the want of which may cause my conclusions to appear “most lame and impotent.” But let it be remembered, that when we indulge in the desire of producing well rounded periods, accuracy is liable to be sacrificed to rhythm; and evidently often is, inasmuch as what is esteemed a good style of composition, is in fact *blank*

verse. Hence medical authors, as well as others, actually write poetry, albeit they may intend it to be prose. When, however, the author is confined to aphorisms or short propositions, he is forced to state his opinions in a succinct and definite manner, and thus presents them in the most convenient forms to be remembered or discussed: and if discussed, the gain to science is certain and tangible, whether they are proved or disproved; the establishment of a negative as well as of an affirmative proposition, serving as a fixed point which determines the actual boundaries which science has attained, and thus, as it were, clears the ground for further advances.

FIRST PROPOSITION.—*Irritation of the mucous membrane of the stomach is indicated in slighter degrees of intensity by morbid secretions on the tongue, bad taste and odour of the mouth, and frontal headach; sometimes hiccup, and in the greater degrees by thirst, loss of appetite, and heat of the hands and feet.*

Irritation of the Mucous Membrane.—In order to obtain a definite idea of what is to be understood by the term irritation of any surface as contrasted with inflammation of the same, let a portion of the skin be rubbed till heat and redness are produced, it is then in a state of excitement or irritation; and if the same process is often repeated, it not only is re-excited, but becomes more easily exciteable for the future, acquiring a higher sensibility, which causes it to be affected by less powerful stimulants than previously.

This constitutes morbid irritability, a state which although usually leading to inflammation is yet different from it, inasmuch as it may continue without any further change taking place, while in the latter, alterations of structure are in progress, which terminate in ulceration, gangrene, softening, or induration. Neither induration nor morbid irritability of the mucous

membrane can be discovered after death, and even inflammation of the stomach in the first stage, may equally after death present no traces of its existence. In proof and illustration of this, take a portion of the surface occupied by erysipelas, and contrast its appearance before death with that a few hours after death; the redness, however previously intense, will be found to have become much paler, if it has not entirely vanished. In some cases of this nature, in which there were also a number of abscesses under the skin, on examination after death, it was found that not only the superficial redness, but the contents of the abscesses had disappeared, so that no trace of the previous diseased state was to be detected, except the excavations in the cellular structure, in which had been the seats of the abscesses. In the process of putting up pathological preparations, we have occasion to see how fugacious the appearance of increased vascularity is, and especially when immersed in water. In the stomach such appearances are peculiarly liable to become invisible from being exposed to maceration in the mucous and other fluids always contained in that viscus, and during the time which usually elapses before a *post mortem* examination is permitted, it is inevitable that the evidences of irritation, and of the first stage of inflammation, must be greatly diminished if not entirely obliterated. As for increased irritability of the stomach during life, its existence can be shown only by observing the effects of irritants; it is hardly to be expected that any trace of its existence should ever be detected after death.

That the colour resembling crimson velvet, which Doctor Roupel has depicted, from the stomach of dogs poisoned by alcohol, or corrosive substances, is never seen in the human subject, is a fact strongly corroborative of the truth of the above observation, which is one of the highest practical importance, inasmuch as the absence of pathological indications after death in this case, has led to a neglect of the symptoms denoting the true state of the membrane during life, and consequently to erroneous plans of treatment. The great liability of the stomach

to acquire the state of morbid irritability now described, may be inferred from the exposed situation in which it is placed with respect to irritating agents. As the food of which it is the reservoir is not regulated by any fixed rule, but is administered by the depraved or perverted appetite of the individual, or by the external circumstances to which he may be exposed ; and as that food remains on the average about four hours during the process of digestion, the mucous membrane of the stomach is acted on not only by it, but by the new combinations which are developed in it, and hence innumerable sources of injury to the stomach arise within itself. The immediate action of injurious substances is within certain limits averted by the mucus poured forth from the membrane, the quantity of which being in proportion to the intensity of the irritation, a protective covering is usually supplied for the occasion ; hence the mucus has been compared to the cuticle which it resembles in its chemical constituents.

On examining the mucus found in the stomach after death, I have ascertained that although a mixture of it with water will not pass the filter, and although it is not to be considered as soluble, yet that it may be mixed with water by agitation, so as to form a mixture from which it will not readily subside, and that this is equally the case in cold or hot water. From this fact I deduce a practical conclusion, which shall appear in the sequel. There are, however, irritating substances, which, in spite of this protection, affect the membrane, in a short time after being taken into the stomach. Such are all substances soluble in water, which reach the membrane by dissolving in the water which belongs to the mucus : for example, thirst is perceived in a short time after taking salt or sugar, and the means to which we are instinctively led for combating this thirst is the addition of water, which dilutes the contents of the stomach, and thus presents the irritant in a weaker form. Another kind of irritation takes place when cold is suddenly applied to the mucous membrane, as is experienced in taking ice, which is followed by such a reaction

as to cause a sensation of heat and thirst within a short time. I use the word *irritation* to denote the state of excitement immediately produced by the action of stimulants on the mucous membrane; and *irritability*, to denote the state of being easily irritated, which constitutes a disease in the stomach not to be recognized by the local phenomena of pain and heat in the part affected, but by symptoms depending on the sympathies of that organ with other parts.

Morbid Secretions on the Tongue.—The papillæ on the surface of the tongue in the healthy state, are covered with a secretion which is quite distinct from either the saliva or mucus, and which bears a close resemblance to the *smegma* secreted on the corona of the glans penis. It is white, tough, and collects at the extremities of the papillæ. Denis found in fifteen grains of it, in a case of gastritis, five parts phosphate of lime, above one of carbonate of lime, and above seven of a modification of mucus. To this the ordinary white colour is due; and a morbidly increased secretion of it forms the white, creamy, covering of the tongue, so characteristic of fever in its earlier stages. Now, according as the stomach is irritated, the secretion of this substance is altered. This renders the tongue a kind of index to the state of the stomach: why it is so is not easy to explain, inasmuch as the closest inspection of the œsophagus cannot detect in it any such alteration, even in cases of intense gastric irritation. I am aware that M. Bouillaud has given a plate of a white coating of the œsophagus as appearing in children; but I have not been able to verify his description, and am enabled to assert, that the surface of the œsophagus remains unaltered in cases of the most remarkable alterations of secretion of the tongue in the adult. We, therefore, are reduced to the usual refuge of pathologists, when attempting to explain what they do not understand, and must refer it to a sympathy existing between the mucous surface of the tongue and that of the stomach. In sudden irritations of the stomach, no change may be discernible, as a short time is required before the old

secretion is cleared off: but the first alteration usually is an erected state of the larger papillæ situated between the centre and the edge of the tongue. Those, from excess of irritation, cease to secrete, and are consequently of a bright red colour, causing the dotted appearance of the tongue; the red dots being the papillæ, covered only by the saliva. This appearance is almost always to be observed on the tongues of children, and is very frequent in different degrees in adults. When irritation of the stomach is longer continued, then a deficiency of secretion at the centre and towards the tip, or a general increase of the white secretion over the entire surface, without forming the thick coating of the febrile tongue, is observed. In this latter state there is usually a sensation as if the upper surface had been scalded or covered with leather; and the sense of taste is lost, so that the food is taken without any enjoyment. In cases of irritation of the highest degree the secretion of the white matter ceases entirely, and the surface becomes red, an appearance sometimes mistaken for healthy cleaning of the tongue. When this is accompanied by a deficiency of secretion from the salivary glands, then the surface is both red and dry, forming the beef-steak tongue so well known in low fevers.

Bad Taste and Odour of the Mouth.—Those are most predominant in the morning before breakfast, being moreover a frequent consequence of late suppers in persons not habituated to them. The odour in many approaches nearly to that of fæces, except when diversified by that proceeding from carious teeth. That it arises from the secretions retained in the mouth and pharynx, rather than from fresh secretion, appears whenever it is much diminished after cleaning the mouth or taking food. In some individuals, the fæcal foetor of the breath is a constant accompaniment of constipated bowels, and will not disappear till after a due employment of purgative medicines, and in such cases it may be questioned whether the foetor is caused by morbid secretion from irritation produced by fæcal accumulation, or by absorption and subsequent exhalation of the odorous con-

tents of the bowels. The similarity between the two odours favours the latter supposition, especially when we recollect for how long a time, and how strongly, garlic, assafoetida, turpentine and other odorous substances are revealed in the breath, after having been taken into the stomach or rectum.

Frontal Headach.—It has been long observed, that bilious headaches are confined to the temples and forehead, while those which are connected with vascular fulness are towards the occiput. As a general proposition it may be stated, that the headaches which come on with the accession of irritation of the stomach, are situated in the front of the head; they are, in a great majority, in the frontal sinus, and not within the cerebral cavity; this may be inferred from their being unaffected when the patient coughs. There are, however, some cases in which an increase of pain is felt on coughing; in those there is almost always heat of the head, and other indications of vascular fulness in the membranes of the brain. Those two circumstances, viz. heat of head, and the effect of coughing increasing the pain, are of great importance in forming a diagnosis as to the nature of headach. It often happens that the patient regards the headach alone as constituting his ailment, and overlooks the other symptoms of irritation of the stomach. Thus we often hear of headach being cured by taking ice, or large draughts of either hot or cold water, or by other agents taken into the stomach, which would be nugatory if that organ were not the part primarily affected.

Often Hiccup.—In infancy hiccup is most frequent, and arises often from mechanical irritation, such as that of the food being shaken in the stomach by exercise after meals; we occasionally witness its occurrence in connexion with the irritation of acids in the stomach, in which case it is appeased by alkalies; in other cases it is appeased by vinegar; and in Dr. Duncan's paper on Diffused Cellular Inflammation, we have an instance of the utility of constant administration of warm milk and water in appeasing it after other means had

failed. Hiccup occurs in some after the slightest irritations of the stomach ; but in general when occurring in adults not previously affected by it, and accompanied by lowness, or intermission of the pulse, flatulence, vomiting, dryness of the tongue, and a lurid aspect of the countenance, it denotes that kind of irritation of the stomach which comes on when there is great prostration of the vital forces, as in gangrene, and accordingly is always a formidable symptom when thus combined. I had lately a remarkable case of hiccup, in Mercer's Hospital, the appetite and all the functions were in their usual state, but the hiccup was pertinacious day and night, not intermitting during sleep, the subject being a stout middle aged man. The cause of it was not ascertained further, than that there had been a previous exposure to cold. After an ineffectual use of mercurials, opiates, purgatives, and counter-irritants applied over the stomach, it appeared to have been not at all connected with that organ, but rather with the brain and nervous system, as it at once ceased when a blister had taken effect on the occiput, and never afterwards returned. I mention this case along with the preceding observations, in order to shew from what various causes hiccup may arise, and that while it is absent in the greater number of irritations of the stomach in adults, it occurs sometimes from very slight irritations ; and again, that while it justly excites great alarm when coming on along with vomiting and sinking of the vital forces, it may also occur independently of any affection of the stomach whatsoever.

Thirst.—A very constant symptom of acute irritation, although diminishing when the affection has been of long standing. In health, the desire of taking liquids depends much upon habit ; some drink water every night before going into bed, and rise at a certain hour to take it again ; while others take so little drink, that there is a scanty secretion from the kidneys, and that not sufficient for carrying off the salts. This latter defect is prevalent amongst females in the better classes of society, who often abstain almost entirely from drinking at

dinner. In irritation of the stomach, from whatever cause, the alleviation of thirst by water alone is generally unsatisfactory, except the water be taken at the temperature 98° , in which case no reaction from difference of temperature arises, and the dilution of the contents of the stomach produces relief, as has been already mentioned; but the most complete alleviation of thirst is obtained from astringent fluids, as solutions of alcohol, &c. by which this specific sensibility of the mucous membrane is for the time diminished, and a more complete alleviation obtained. Hence the thirsty are prone to refresh themselves with the fluids which possess this mode of action; but for the most suitable drink in cases of diseases, I refer to the treatment.

Loss of Appetite.—That is of the healthy appetite. In some cases the patient eats, but without any relish or enjoyment; he describes it as if he was eating so much *chopped straw*, or other tasteless substance; he is yet induced to eat by a sense of exhaustion, which comes on at the habitual hour for eating, and will sometimes eat more than usual, but it is a feeling of a want of something rather than appetite, which urges him to do so. A stomach in a state of irritation will also at times suggest a desire for stimulating food, to which at other times the patient had been unhabituated. Thus in fever we are occasionally asked to allow red herring, or cheese, or salt beef, while those mild articles of diet which alone can be admitted, are regarded with aversion; hence, also, many, ignorant of the truth that they are actuated by disease, are occupied perhaps, during a great portion of life, in ministering to the morbid cravings of an irritable and constantly irritated stomach, when they might under an opposite course of diet, come into the enjoyment of a healthy appetite, with all the advantages attendant thereon. This desire of irritating substances by a stomach in a state of irritability, resembles the sense of itching in an already irritated portion of skin, and if indulged in either case, the irritation is rendered more intense, and more apt to end in inflammation.

Heat of the Hands and Feet.—As the stomach is insensible to the sense of touch, which, if existing, would most probably cause a constant direction of our thoughts to it, to the neglect of every thing else, so is it destitute of those other sensations which announce the existence of irritation on the surface of the body. For example, titillation or itching has no place here ; no one has ever desired to scratch his stomach, or any part of his intestinal canal, and it being thus destitute of the local indications of irritation, we have to seek for them elsewhere. Amongst the organs which are affected by irritation of the stomach, must be placed the heart, which so readily answers to the administration of stimulant liquors, that it is capable of being roused to action in this way, when in a state of syncope, and which in the ordinary state is always sensible to excitement of the stomach, which also is no less depressed from changes in the same organ, insomuch that the suddenness of death from the acrid mineral poisons, arises from the closeness of this connexion, rather than from any other cause which can be assigned. Much, however, as the stomach influences the heart, it influences the capillaries still more ; and the most remarkable evidence of this is presented by the symptom now before us. The heat is felt in flushes at the soles of the feet and palms of the hand, while the rest of the body may be actually cold ; I have often received accounts of heat of the feet coming on at night, forcing the patient to keep them outside the bed clothes, as the principal, if not the only subject of complaint ; while the true nature of the disease was not discovered till after a searching inquiry into the symptoms already enumerated. Of such importance is it to recollect that irritation of the stomach is not so much to be recognized by sensations in the part affected, as by the irritation which it excites in distant parts.

SECOND PROPOSITION.—*The causes of acute irritation are :*

1. *The presence of substances acting chemically on the mucous*

membrane ; 2. Fæcal accumulations in the intestines ; 3. Mental excitements ; 4. Sympathy with other organs ; 5. Hysterical sensibility ; 6. Sinking of the vital forces.

Substances acting chemically on the Mucous Membrane.—To enumerate the various substances which enter into the articles of diet and of medicine, and which irritate the mucous membrane by their chemical agency on it, would require too large a space ; suffice it to state, that they are for the most part soluble in water, and as has been mentioned before, thus obtain access to the membrane. Those articles of diet which contain sugar, salt, alcohol, or acids, are all proportionably liable to produce irritation. The same effect may be produced by insoluble substances, but not in the same way ; for instance, salmon, salads, or cucumber, first act on the gastric glands, and (as is proved by the experiments of Gmelin and Tiedemann) cause them to pour forth so large a quantity of sour fluid, as to at length be intolerable to the stomach, and thus heartburn and vomiting ensue ; this may continue for a long time, and serious mischief be averted by means of the mucus poured forth by the membrane ; but in process of time it is from frequent excitement rendered more and more irritable, till at length inflammation and ulceration are produced ; when this has taken place, then both the symptoms of irritation of the gastric glands, and those of irritation of the mucous membrane, will be set up in the same individual, and that in an undeviating and permanent form. But of this combination of two diseases, I shall have to treat hereafter.

Fæcal Accumulations in the Intestines.—Those are more frequently productive of irritation of the gastric glands, as denoted by sourness and distention ; that of irritation of the mucous membrane, except in the young, and especially in infancy, in which the latter affection is by far the most usual consequence of constipated bowels.

Mental Excitement.—Well known to occasion thirst and

anorexia, which continue for some time after the occasional cause has ceased.

Sympathy with other Organs, &c.—Under this, and the two following heads, are included agencies, acting within the system of the individual, and consequently not to be determined, except with reference to the peculiar circumstances of his constitution. In many of these, not actually irritation, but only irritability is produced, and that of a temporary character, while in others, as in prostration of the vital forces, a peculiar state of irritation is excited, and the patient is assailed with hiccup and vomiting, even though there be nothing present in the stomach, and no reason either during life, or after death, to infer the existence of any primary disease of any part of that organ. It must be evident, that the study of the stomach is no less difficult than important, when we see one set of phenomena produced by its influence on remote parts, and another by the action of remote parts on it. The correct interpretation of these phenomena can be obtained only by the investigation of a great number of cases, placing them in various combinations, so as to display their mutual dependance.

THIRD PROPOSITION.—*Morbid irritability (chronic irritation) of the stomach is denoted by the following symptoms : sense of fulness or dull pain in the region of the stomach ; general uneasiness and moroseness ; flushes of heat in the extremities ; dulness or chilliness ; the above commencing immediately after taking food, and continuing for an uncertain period within four hours ; tongue foul, especially in the morning ; appetite capricious or extinct ; thirst ; nausea ; insipid vomiting ; ptyalism ; frontal headach : each case presenting more or less of the above symptoms, according to the nature and degree of the irritation, and the constitution of the patient.*

Sense of Fulness or dull Pain.—A result of the excited sensibility of the stomach, which in health ought to communicate no sensation of the presence of food, further than the

appeasment of appetite. In this state it is often painfully sensible, and always suggests the idea of an elastic tumour, which does not subside till the greater part of the process of digestion has been completed. This feeling of distention differs from that accompanying irritation of the gastric glands; 1st, in not being accompanied by sour eructations, the gas, when eructed, being inodorous and insipid, or else resembling rotten eggs, from the presence of sulphuretted hydrogen; and 2ndly, in coming on almost immediately after taking food, while, in the other case, it does not commence until one or two hours.

General Uneasiness and Moroseness.—Those sensations are set down here, although not peculiar to the stomach, and occurring in irritations of the whole tract of the small intestines. How the temper of an individual should be affected by the state of his stomach and bowels, is a question no less difficult for the psychologist, than the physiologist. The fact cannot be denied. Many an unhappy frame of mind, rendering the individual waspish and unaimiable in the highest degree, is terminated by a spontaneous purging of dark-green liquid matter. To this connexion between low spirits and dark coloured dejections we must ascribe the formation of the Greek word melancholy, from words signifying black and bile. The feeling of distention above mentioned, is always accompanied by moroseness, or at least, by heaviness and indisposition to exertion of body or mind. The usual frontal headach, the want of proper appetite for food, and a feeling either of fulness or emptiness, and exhaustion dependant on the distention, combine to produce a kind of misery *sui generis*, to which the term pain is inapplicable, but which causes the sufferer to bestow exclusive attention on those internal sensations, and unfits him for any thing else. In this state, cheerfulness cannot be extracted from any thing. All around appears but “a pestilent collection of vapours,” and a tendency both to take and give offence has suggested to Voltaire the observation, that most of

the wars of Europe might have been prevented, if ministers of state had attended to the proper regulation of their digestive organs.

Chilliness.—A feeling of cold after dinner is, in a slight degree, common to persons in health, but in this case it is accompanied by coldness of the extremities, alternating with flushes of heat in the palms of the hands and soles of the feet, and by the other sensations already mentioned. In some cases the chilliness almost amounts to a rigor, and resembles the same phenomenon as produced in irritations of the bladder.

Insipid Vomiting.—The matter vomited is insipid, except in as far as it tastes of the food last taken, or of bile regurgitated from the duodenum, because the taste of mucus is insipid. This is often a very marked distinction between vomiting in irritation of the gastric glands, and in irritation of the mucous surface, it being sour in the former.

Ptyalism.—An occurrence not uncommon during pregnancy. I had a patient, a lady, who, during her first pregnancy, was much harassed by nausea and vomiting, but in two subsequent ones, was completely free from them. On both those occasions, however, a violent ptyalism came on, which resembled that produced by mercury, except that there was no fœtor or sore mouth. This compelled her to retire from society, for three or four months. She tried several remedies, but none were of permanent advantage, and it continued till her confinement took place, when it completely ceased. Here was an instance of uterine irritation producing in the same individual, at one time vomiting, and at another ptyalism. The increased flow of saliva, when the appetite is excited, is familiar to every one, but the connexion of ptyalism with irritation and chronic inflammation of the stomach, although noticed by some French writers, has not been generally attended to or acknowledged. Yet I have seen cases of it. In some of them, to a superficial observer, ptyalism was the only symptom, and it was only by a close examination of the case, and of the result and treatment,

that it was rendered evident, that the cause of the ptyalism was irritation of the stomach. It may be here observed incidentally, that mercury produces ptyalism, not so much by an immediate action on the glands, as by a previous irritation of the mucous membrane. For, 1st, it does not produce it more rapidly when applied on the salivary glands, as it should, if acting directly on them ; 2ndly, it causes soreness of the mucous membrane of the mouth before the ptyalism commences ; and 3rdly, when carried on to excess, it often produces ulceration of the mouth and intestinal canal, while the salivary glands are but slightly affected, but never the contrary.

FOURTH PROPOSITION.—*The causes of chronic irritation are the same as those producing acute irritation, acting with less intensity but more constancy.*

FIFTH PROPOSITION.—*The treatment of both acute and chronic irritation of the stomach, is to be directed, 1st, to the removal of its local causes, by the substitution of an appropriate diet, and 2ndly, to calming the irritation already existing. The latter is affected by, 1st, dilution of the contents of the stomach ; 2ndly, diminution of its sensibility, by the agency of cold or heat ; 3rd, the use of astringents in sedative doses, as acetate of lead, sulphate of lime, nitrate of silver, lime water ; 4th, stimulating the circulation of the skin, by means of external irritants, bathing, frictions, and exercise ; 5th, stimulating the circulation of the pulmonary surfaces by changes of air ; 6th, stimulating the nervous system by mental exhilaration.*

The Removal of the local Causes by the Substitution of an appropriate Diet.—When the mucous surface is irritated, it must be looked upon as in the same condition as an irritated portion of skin, with regard to topical applications ; and those substances which are most appeasing to the latter in the form of poultices, are also to the former, as articles of diet. The food then should be vegetable, and consist chiefly of

amylaceous substances, as rice well boiled, arrow root, &c. stirabout taken in moderate quantity, with milk diluted. It is, however, to be observed, that stirabout lies under the imputation of causing heat and itching of the skin, and justly so, those affections being peculiarly prevalent where oatmeal is much used. It is to be ascribed to a resinous matter, in the covering of the grain soluble in alcohol, which has been ascertained to produce this effect on the skin. When meats are taken, they should be such as are most free from osmazome, as chickens, rabbits, sheep's trotters, &c. and should always be accompanied by boiled rice or other vegetable matter, in order to diminish the stimulating effect. Of all animal substances, that which appears not only to have no stimulating effect, but actually to appease the stomach, is the yolk of eggs. I was informed some years ago, by Mr. Daniel Moore, of the case of a lady labouring under pertinacious vomiting, which he completely cured, by frequently repeated spoonfulls of raw yolk of egg, although it had been previously treated ineffectually by a variety of the most approved remedies. Amongst the cases attached to this communication will be found an instance of vomiting, from a congested state of the stomach in the last stage of diseased mitral valve, in which a similar success attended its employment. I could have added several others, and my failures with it have been principally in hysterical cases, in which, perhaps, it is too disagreeable and disgusting to be retained sufficiently long, to exercise its soothing properties. Eggs, in the form of light-made custard pudding, are commendable ; but having mentioned puddings, it must be remembered, that all combinations of flour and butter, in which the latter has to be subjected to heat, and then to be rendered rancid, are not only indigestible, and thus apt to provoke a large secretion of sour fluid from the gastric glands, but are also in a high degree irritating to the mucous membrane. Such are various kinds of pastry, and to these may be added, the rancid oily nuts.

The articles to be avoided in these cases are, salt and sugar

in their various combinations. Even tea should be taken without sugar. In short, let the patient suppose at each meal, that he is going to apply a poultice to the interior of his stomach, and he will not go far astray. Let him also not overload, but eat slowly, and about four times in the day, because mere weight, acts as a mechanical irritant, and is felt as a "load in the stomach." A habit of eating quickly is productive of overeating, hence, slow mastication must be strongly insisted on, and in case of defective teeth, the food must be taken still more slowly, and in a comminuted form. Those observations appear trifling, but when we reflect, that the food has to come into actual contact with the irritable surface, it cannot but be deemed of the highest importance, that it should be presented in the least irritating form. In the whole series of stomach complaints, the physician must have the direction of every thing which goes into the stomach, whether as food or medicine, and to be of real use to his patient, he must be no less skilled in cookery than pharmacy.

Dilution of the Contents of the Stomach.—The effect of water taken into the stomach, is to diminish the irritation of its contents, by diluting them. Hence it is the usual custom to drink towards the conclusion of a meal. Another use of dilution, and that which renders it a medicine, is to dissolve the mucus, and thus to render the membrane accessible to astringent remedies. This effect is best obtained by exercising after drinking largely of warm water. The mucus is thus mixed up and dislodged. According to my view, this mode of action explains the efficacy of mineral waters in irritability and chronic inflammation of the stomach. When an invalid at one of the German spas drinks six or eight beakers of the water before breakfast and walks in the intervals, he washes out the mucus, (as I have proved can be done in the dead stomach,) and thus the small proportion of salts held in solution, acts on the membrane as sedative and astringent. This view has to me been confirmed by experience, as I find, that by administering one or more

tumblers of warm water in the morning, then after exercise, giving the sedative astringents to be hereafter mentioned, the most striking results can be obtained, and such as are usually only witnessed during a course of mineral waters.

Another kind of dilution, is to mix mild along with stimulating articles of diet. This is practised at our daily meals, when we use bread or vegetables in alternate mouthfuls, with meats or seasoned dishes. An experienced gourmand at a feast, will even venture to take things which he knows to disagree with each other, if he has access to good stale bread, as by taking a quantity of this, he is enabled to interpose such a *substratum* as shall prevent them from coming into immediate contact.

The Use of Astringents in sedative doses, as Acetate of Lead, Sulphate of Zinc, Nitrate of Silver, Lime Water.—I merely state a fact, and am not proposing a theory in describing astringents as a sedative to the mucous membrane when applied in small doses. Let this be judged of by the effects produced, which are diminution of redness, of heat, and of sensibility. Such effects are well exhibited in the most approved applications to the conjunctiva of the eye, and in gargles and lotions injected into the urethra. When the same substances, however, are applied in an undiluted form, then chemical decomposition and destruction of the membrane ensues. Seeing then the importance of attending to doses in these cases, I shall state here, the forms in which I am in the habit of prescribing those substances. They are as follows. 1st.

℞ Superacetat. Plumbi grana xii.

Aceti ℥ss.

Aquæ destillat. ℥ viii. M.

Sum. coch. ii. ampla mane et meridie.

To the above mixture, an addition may be made of half a grain of acetate of morphine, in case of general restlessness, or in case of the mucous irritation being excited or aggravated by excessive secretion of sour fluid from the gastric glands. In order to

give the above and the two following formulæ their full effect, the patient should drink a large tumbler of tepid water on an empty stomach, and use a little gentle exercise before taking the medicine. As for the danger to be apprehended from the preparation of lead taken in this way, I can state, that although now for years in the daily practice of writing the above prescription, I have never seen one instance of either colic or paralysis resulting therefrom.

The acetate of lead formula I use in the earliest and more acute cases. Next in point of astringency, and more applicable to chronic cases, is the following :

℞ Sulphat. Zinci gr. iv.
Aquæ destillat. ℥ ii.

Sumat coch. ii. ampla post aquæ calefactæ amphoram vacuo ventriculo mane et meridiæ.

The last of these formulæ, and that most to be depended on in chronic cases, which has also the advantage of being well suited to the cases of ulceration of the stomach described in my former paper, is the following :

℞ Nitrat. Argenti grana ii.
Sacchr. alb. gr. iv.

Ft. pil. viii. Sumat i. mane et meridiæ. With the same directions as the two former.

The success obtained in hysterical and convulsive affections by nitrate of silver, appears to me to be derived from its astringent and sedative effect on the mucous membrane of the stomach. It being decomposed very shortly after its arrival there by the free muriatic acid, and by the muriate of soda, causes it to change its properties before it descends into the bowels, where it may eventually prove a laxative.* Both in chronic irritation

* The silver pill of Boyle, or *angelus sala*, was composed of equal weights of nitrate of silver and nitrate of potash, thoroughly mixed together with the aid of heat. Boerhaave (Chemistry by Shawe, vol. ii. p. 297) gives the following observation :

and inflammation of the stomach it is an heroic remedy, the effects of which only require to be aided by a judicious attention to diet and the management of the bowels, in order to inspire the patient with the highest degree of confidence in the resources of the medical art. The discoloration of the skin, which causes female patients to regard the internal use of nitrate of silver with great apprehension, I have never yet witnessed in any of my patients. The above mentioned dose, from being speedily decomposed, appears to act merely as a topical application to the stomach, and as I am not in the habit of continuing the use of it longer than a week at a time, the absorption of it in an undecomposed form, and consequent deposition of silver at the skin, appears next to impossible.

The last article mentioned in the proposition is lime water. This is to be taken abundantly as a drink, and mixed with milk or barley water. I might also have included the decoction of catechu and of logwood, the latter especially, as being an agreeable drink; but I have preferred to mention only those articles which manifold experience has stamped with a peculiar value in my treatment of this complaint, and leave to the judicious reader to supply the rest, by always bearing in mind that irritable or inflamed surfaces in the interior resemble those in the exterior with regard to those substances which come in contact with them.

The fourth, fifth, and sixth heads embrace matters of the utmost importance, and without which all others will prove ineffectual in this complaint. Without exercise in the open air, I have rarely seen permanent benefit obtained, and even at the risk of

“ If a little of it be applied to ulcers, it acts like *lapis infernalis*, only much milder, and if two grains of it be fine ground with six grains of loaf sugar in a glass mortar, then mixed with ten grains of the crumbs of bread, and formed into nine pills, and these be taken by a grown person upon an empty stomach, drinking after them four or six ounces of hot water sweetened with honey, they will purge gently, and bring away a liquid water, that often deceives the patient, as coming away almost without being perceived.”

cold and damp weather, the patient must go out every day. Under stimulation of the skin, is to be placed an application to which I attach a high value, namely, that of mustard to the region of the stomach. The flower of mustard simply mixed up with water applied on a cloth over the stomach every evening, and suffered to remain on till smarting is produced, is a powerful adjuvant, and interferes with no other remedy. It also gives a temporary relief from the pain coming on after dinner, and is generally most acceptable to the patient: it requires no dressing except dry lint or wadding. On the head of mental exhilaration much might be said. Here the *medicina mentis* becomes a most powerful auxiliary. Many physicians know no other remedy of this kind but to send the patient to travel, forgetting how often it proves true that *cælum non animum mutant qui trans mare currunt*, and that in the stores of the imagination, even when remaining at home, there are many resources by which a healthy interest in life may be excited, alternate employment and relaxation provided, and hopes and expectations for the future enlivened. Of such importance is this, that the patient will not only imagine, but will, at least for a time, actually derive more benefit from a practitioner of high repute, whose words sound like oracles, than from one of inferior eminence, although the prescriptions of both may be the same.

SIXTH PROPOSITION.—*Acute inflammation of the mucous surface, when most intense, is accompanied by the same symptoms as acute irritation but in a more permanent form, the vomiting more severe, and sometimes accompanied by constriction of the cardia, preventing the discharge of the contents of the stomach. There are also general febrile symptoms, with a tendency to temporary remissions and exacerbations. Pain on pressure may or may not be present.*

Constriction of the Cardia.—This is most frequent in cases of poisoning by arsenic, and one reason of the greater certainty

of operation of that substance when administered with a murderous intention. Several other poisons of the same class are promptly rejected by vomiting, but in the case of arsenic the fluid rejected is usually only a ropy fluid consisting of the mucus derived from the pharynx and œsophagus.

Temporary Remissions and Exacerbations.—Belonging to inflammation of every portion of the intestinal tube as well as the stomach, and arising from its peculiar mode of sensibility, causing those sympathetic disturbances in distant parts which have such a tendency to divert the attention from the real seat of the disease.

Pain on Pressure may or may not be present.—To determine that there is no inflammation because there is no pain on pressure is a very common mistake, and may prove of serious consequence. That it is a mistake every one must be fully aware, who has had the opportunity of comparing symptoms with appearances on dissection. Often have I found no pain on pressure, while the stomach and small intestines were not only inflamed, but extensively ulcerated; and even in the severe cases of dysentery, it appears to come on only in proportion as the peritoneum is engaged in the disease. We must also, with respect to the stomach, distinguish between pain on pressure, and that tenderness arising from mere distention, which exists even in health, when the stomach is distended from flatulence, or after a full meal, and which must be greater when the mucous membrane is in a state of excited sensibility.

To be continued.

ART. XXIII.—*Postscript to Dr. GRAVES'S Observations on the Prevalence of Fever in Ireland.*

THE statements made in my paper are still further confirmed by a letter since received from that eminent physician, Doctor Little of Sligo, of which the following is an extract.

“ During the last year I can state that not one case of pure

fever has occurred among the higher classes in this town or vicinity, except one of a very anomalous character, in the person of an elderly lady who died under a marked cerebral combination. Our Fever Hospital here does not afford a very correct standard of the state of fever in the town or suburbs, for there is a strong prejudice amongst the lower population against it, which the very title 'Fever Hospital' is calculated to excite and maintain, and it is rather supplied from the adjoining villages and surrounding country ; but it is a positive fact, that there has not been a year for several years past, so marked as this by immunity from fever. In the year 1836, there were many cases of fever, where the disease had considerably changed its type from that of the preceding years, in so much, that the medical men who were the strongest advocates for the general bleeding plan, were obliged to change their practice, and confine their bleedings to the local complications of the disease, while the early exhibition of stimulants was strongly indicated ; and such miraculous recoveries have I seen under the full exhibition of brandy and wine *ad libitum*, with musk, camphor, and ammonia, as have brought to my mind the days of my youth, when a similar practice was generally adopted, and frequently carried too far, so as to reduce the patients to a state of intoxication. In the fever hospitals here, they have given a full trial to the tartar emetic and opium in delirium, and have found it fully to answer their warmest expectations. Dr. Homan, who is a very intelligent practitioner, speaks very highly of the practice. There is a great difficulty in obtaining *post mortem* examinations, but in such cases as they have been made, cerebral and thoracic lesions have been more frequently observed than abdominal."

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An Outline of the History of Medicine from the earliest historic Period to the present Time, intended to illustrate the Connexion between the Progress of Anatomy and the Improvement of the Healing Art. By PHILIP CRAMPTON, F. R. S., Surgeon General to the Forces in Ireland, and Surgeon in Ordinary to the Queen.

[Read before the Royal College of Surgeons on Thursday, the 29th of November, 1838.]

AN inquiry into the origin and progress of any department of human knowledge must ever be a subject of the utmost interest and utility ; for while it gratifies curiosity, (the first and simplest emotion of the human mind,) it serves to throw some light on a matter, which of all others concerns us the most, the means by which a large portion of the human race has been enabled to emerge from a state of ignorance, misery, and crime, to one of comparative knowledge, happiness, and virtue.

But the progress of knowledge, like the progress of the human mind, (of which it is but the expression,) has never been uniform ; in some ages and in some countries, it has advanced with a continually accelerating velocity ; in another it has become stationary or even retrograde ; again, after centuries of inaction, as if invigorated by repose, it springs forward with an energy which seems to give promise of an eternal progression, and almost tempts us to believe, against the evidence of our experience, that a sun which has risen, in such brightness, will never again set in clouds and darkness. The hope may be fallacious, but not so the practical inference which this view of the subject suggests ; for, if it be true, that the course of human knowledge be so uncertain, we are naturally led to inquire into the causes of this uncertainty, and it is from history alone (that “philosophy which teaches by example) that we can obtain an answer to our inquiry.

It must be admitted, that the causes which either accelerate or retard the progress of human knowledge, are for the most part either concealed from our view, or placed beyond our con-

trol ; we look in vain for the cause of those great political convulsions which so often change the whole constitution of society, generating new tastes, new modes of thinking and of acting—here converting a nation of rude warriors into philosophers, and there converting philosophers into warriors. We may deplore, but we cannot arrest, the torrent of barbarian conquest which may again sweep over the fairest and politest regions of the earth, remove the very landmarks of civilization, and convert our cities, those strongholds of liberty, those sacred abodes of the sciences, and the arts, into “a howling wilderness.” But least of all, can we produce those master spirits, those rarest and best of Nature’s gifts, which appearing at long and uncertain intervals, seem formed not only to illustrate their own age, but to give an impulse and a direction to human knowledge, which sends it forward to bless and to enlighten ages yet unborn. Over such causes we have no control, but there are *some* which fall so clearly within the scope of human influence, that it becomes an object of the deepest interest to ascertain their nature and mode of action. To investigate those causes, so far as they seem to bear on the progress of the healing art, is the principal object of this discourse.

To render this inquiry in the least interesting, or even intelligible, to those who are unacquainted with the history of medicine, it is necessary that I should give a sketch (it can be no more) of the origin and progress of the art, from the earliest historic period to our own times. To do justice to such a subject would require qualifications very different from those which I can bring to the task, even if the extent and variety of the details did not preclude the possibility of bringing them within the compass of a single discourse.

With such difficulties to contend with, I should certainly have relinquished a task, which I have perhaps rashly undertaken, if it had not occurred to me that in doing justice to the subject, I should commit an act of the utmost injustice to my auditory. That auditory consists of our distinguished guests, and the members of our own body. The members, in common with myself, stand here somewhat in the character of hosts, and in this capacity they no doubt feel, (like myself,) bound to provide for the amusement and instruction of their guests, rather than our own. It would be hard to decide then to which section of my auditory I should give most cause of complaint, if I were to enter upon a critical examination of the lives and writings of the great masters of the art, who have flourished during a period of nearly 2300 years ; our guests would find the disquisition insufferably tedious, from having had no previous acquaintance with, and but little interest in the subject ; and

my colleagues would find it equally so, though from an exactly opposite reason.

A history of medicine, like every other history, should (to be useful,) be given in the spirit of the strictest impartiality; such, however, has unfortunately not been the case, in the instance of the healing art. An extravagant admiration for the ancient writers, has enabled some to discover in Hippocrates and Galen the rudiments, if not the full development, of every discovery which has been made in more modern times; while others, falling into the opposite extreme, find the *present times*, that is to say, the times which they illustrate by their genius, the only times in which any great things have been done towards the advancement of the art which they cultivate. These opposite modes of thinking are what Lord Bacon calls the "peccant humours of literature," of which (he says) the chiefest is the extreme affecting of two extremes,—the one antiquity, the other novelty. Antiquity envieth that there should be new additions, which it may be troublesome to master; and novelty cannot be content to add, but it must deface. "Surely," (he adds,) "the advice of the Prophet is the true direction in this matter, 'state super vias antiquas et videte quænam sit via recta, et bona, et ambulate in ea.' Antiquity" (he adds) "deserveth this reverence, that men should make a stand thereupon and discover what is the best way, but when the discovery is well taken, then to make progression."

With every desire to avoid "the extreme affecting of either extremes," whether in behalf of the ancients or the moderns, I cannot promise to escape the "infection" of the times, no more than I can promise to escape any other epidemic infection, and in declaring that I *intend* to discharge the task which I have assigned to myself, with impartiality, I am aware that I give no security for my doing so; I say no more than every man, not purchased by a party, says under the same circumstances, for no honest man intends to be partial, or is conscious that he is so; but when he is "biased" he declines from the right line, like the bowl, (from whence the metaphor is taken,) because "it is weighted on the one side." But if I can give but slight security for my impartiality, I can give still less for my judgment; I can, however, supply a corrective for any defect which may inhere in either. I will in every case give my authority for any statement which I may make; and if called upon to express an opinion upon the value of any discovery, or on the respective claims of the discoverers, I shall take care to support my opinion by facts, generally taken from the experience of others, but, at all events, facts which are capable of a ready verification.

It would be an unpardonable waste of time to do more on the present occasion than allude to the labour and learning which have been bestowed on the inquiry respecting "The Origin of the healing Art;" those who feel any interest on the subject, may satisfy their curiosity by a reference to the work of the learned and ingenious M. Le Clerc: they will there find the question as to the divine origin of medicine gravely discussed, and the various claims of Apollo, Hermes, Trismegistus, Isis and Osiris, Bacchus, Prometheus, Esculapius, and Adam, as inventors of the art, satisfactorily adjusted.

It must be obvious, however, upon the slightest consideration, that the practice of medicine must be coeval with the origin of disease, and that consequently the first man must have been the first physician. His practice, to be sure, could not have been very complicated, or his experience very extensive, but it is highly probable, that under the guidance of simple nature, it was far more rational and successful than the practice of his successors, with all the aid which it could derive from the so called philosophy and religion of the after ages. If Adam cut his finger, and saw the blood flowing from the gaping wound, it is nearly certain, that he would bring its surfaces together, and keep them in contact by some sort of ligature, until they became united; and if he found the part to become hot and painful, it is highly probable that he would bathe it with water, taken from the nearest spring, and there is little doubt, that a wound so treated would be perfectly healed in three or four days. Such, however, has not been the practice of his successors, in a more advanced, or rather a more complicated state of society. Superstition would perhaps suggest, that the wound was inflicted by the deity, in revenge for some crime committed, or some rite unobserved, and the treatment would necessarily devolve on the priest, whose superior knowledge and sanctity rendered him the proper interpreter of the intentions of the god with whom he was in constant communication. It would be a dire offence to the deity, under such circumstances, to bind up the wound at once, expiation must first be made, the god must be appeased, prayer and sacrifice must be performed, and the wound, covered with sacred balm, would be permitted to heal, after having undergone the painful and tedious process of inflammation and supuration.

Such was the practice of the priests, who officiated in the double capacity of priests and physicians in the temples of Esculapius, at least up to the period of Antoninus Pius.

In an after age, when philosophy somewhat rudely took

the practice of the healing art out of the hands of superstition, the condition of the wounded man was not much improved. "If," says the philosophic surgeon, "a sanious humor be generated and confined in the wound, no one can say what fatal consequences may ensue. The wound must therefore be healed *secundum artem*, and go through the regular stages of inflammation, suppuration, mundification, granulation, and cicatrization."

To produce these happy results, a tent must be pressed down to the bottom of the wound, and kept there by various contrivances, until nature, stronger than perverted art, forces it outwards, and closes the wound in despite of the surgeon. How general the opinion was during the middle ages as to the danger of healing a wound without tenting it, may be collected from various passages in Shakspeare, and other of the older writers. The meaning of the "intended woundings of a father's curse," in King Lear's awful imprecation, will occur to every surgeon, although it has puzzled some commentators. And here I may mention, that the wondrous cures which in the middle of the sixteenth century were said to be effected "by sympathy," that is, by applying to the instrument which inflicted the wound, the ointments and lotions which, by the ordinary rules of art, should have been introduced into the wound itself, were actually effected by returning to the practice which I have ventured to trace up to Adam, of closing up the wound and keeping it wetted with water or some cooling lotion.

Old Wiseman, who was surgeon to Charles I., in speaking of the advantage of uniting simple wounds, which he says, "is an unspeakable commodity, which the whole art receiveth thereby," adds, that "it is this which giveth such credit to the sympathetic powder."

Parcelsus, the alchymist, the cleverest and most impudent of quacks, was the fortunate inventor of this "the sympathetic method" of treating wounds; here is the work, the *Theatrum Sympatheticum*, a very rare and amusing book. It is the opinion of Wiseman, and of many well informed writers on the history of medicine, that the success which attended the sympathetic cure of wounds in the hands of empirics led to the adoption of the simple plan now universally employed by the regular faculty, of uniting wounds by what is termed the first intention.

It requires but little sagacity to perceive that it was the mere bringing the surfaces of the wound into contact, and keeping them so, which effected the cure, and not the application of salves to the instrument by which the wound was inflicted. I find accordingly in another very curious book,

“ The three exact Pieces of Leonardo Phioravante, his rational Secrets in Chirurgery,” that the practice is actually recommended of treating wounds without tents ; he admits that he states facts that must appear incredible, but calls God to witness the truth of his statements. (See page 80.)* And here I cannot but remark how happy an illustration this passage affords of the advantage of employing in a science of observation, such as medicine, observation and experiment instead of speculation ; for had not the minds of medical men been preoccupied by a false philosophy, it is not conceivable that a fact so obvious, and so intimately connected with the welfare of mankind, should have been concealed from their eyes for upwards of five hundred years.

In this account of the treatment of a wound in the different ages of the world, I have by anticipation given a sketch of the history of medicine, so far at least as relates to the description of persons by whom it was exercised, and the influence which the character and education of those persons had on the art itself.

1stly. You have what may be called *natural or instinctive medicine*, in which every man was his own physician, and where there were no persons set apart as practitioners of the art ; this is a state in which medicine exists to this hour in many regions of the earth, and even in this country there are many populous districts that are without any other medical aid, than that which the uneducated inhabitants supply to one another. In those remote districts, however, the clergy often perform the offices of the healing art, but in a very different spirit from that in which it was exercised by the priests who officiated in the temples of Esculapius.

2ndly. You had the “ *medicina sacra*,” or medicine as practised by the priests of whatever religion.

3rdly. You had “ *philosophic medicine*,” or medicine as it was practised in Greece, previously to the æra of Hippocrates, to whom Celsus ascribes the merit of having “ separated medicine from philosophy,” a merit to which I shall have occasion to shew he was not entitled.

Lastly, you have “ *empirical medicine*,” or medicine, very much as it is practised amongst us at this day, in which close attention is paid to the *signs* of disease, rather than to their *proximate causes*, and in which medicines are exhibited, with refe-

* Here Mr. Crampton read an extract from Phioravante, in which he describes the method of uniting wounds by adhesive plaster or bandage, and without tenting, and declares that “ *it is a most rare secret.*”

rence to their known *effects* rather than to their *occult qualities*. This custom of discarding hypotheses, as to the *causes* of disease, and the *modus eperandi* of remedies, is not of very modern date, although it has been revived within almost our own times, and is confined almost to the British nation; it is exactly the doctrine of the empirical sect of which Serapion was the head, and which sprung up shortly after the death of Hippocrates. When I speak of the state of medicine, in the early ages of Greece, I shall have occasion to recur to this subject.

The history of medicine may, I think, be conveniently divided into three epochs. The first extends from the time of Herodotus (about five hundred years before the Christian æra, and about forty years before the time of Hippocrates) to the fall of the Roman empire in the East. I date from Herodotus rather than Hippocrates, because we learn from the former all that we know respecting the state of medicine among the Egyptians.

The second epoch dates from the Arabian conquest to the beginning of the sixteenth century; a century which, indeed, forms a remarkable æra in the history of medicine, for it was then that, through the good offices of the Emperor Charles the Fifth, anatomy received the legal sanction both of the Church and of the State.

The third epoch extends from the beginning of the sixteenth century to our own times.

A critical examination of the state of the healing art, in all its branches, including physiology, pathology, internal and external medicine, during any one of those periods, would occupy many hours, and would, besides, be foreign to the purpose of this discourse, the object of which is merely to take such a general view of the progress of medicine in the different ages of the world, as may help us to form some opinion as to the causes by which that progress has been either accelerated or retarded.

We need not lose a moment in inquiring into the state of medicine among the Egyptians, it is enough for us to know that it was in the hands of their priests and magicians, the ablest, and perhaps the most abandoned men, who have ever lived; by them the healing art was used as a means of increasing their power over the most ignorant and superstitious people in the world, and, consequently, had no reference to its proper object, the relief of human suffering.

The art appears to have been exercised by the chief priests, assisted by the *pastophori*, or image bearers, (an inferior order of the priesthood,) and consisted chiefly in magic rites and prophesyings, and in the empirical use of certain medical agents of no inconsiderable activity, such as senna, aloes, squill, and iron. As little is known of the details of their practice, as of their re-

ligious rites, which it was their policy to involve in the most impenetrable mystery.

The only circumstance worth mentioning, in connexion with the Egyptian physicians, is their ignorance of anatomy, an art which, we might suppose, the practice of embalming, not only human beings, but all the animals that came within their reach, would have enabled them to cultivate with peculiar advantage. It is true that some medical historians, and among the rest, Schultze and Sprengel ; supporting their opinion by the passage in Genesis, in which it is said that "Joseph commanded his servants, the physicians, to embalm his father ;" are of opinion that such opportunities of acquiring anatomical knowledge could not have been lost, and that accordingly the Egyptian physicians must have obtained a great insight into anatomy, both human and comparative, and the learned and ingenious Warburton considers the fact of Joseph having a number of physicians in his suite as a proof of the "grandeur, luxury, and politeness of the Egyptians," at this early period of the world.

But it is by no means certain, in the first place, that the persons who embalmed Israel were physicians, nor in the second, that they were (as Dr. Warburton conceives) "servants" belonging to the establishment of Joseph ; for in Egypt *then*, as all over the East *now*, all subjects are the servants, or rather the slaves of the monarch, and that without reference to their station in the state ; besides, there is every reason to believe that the Hebrew word which we translate physician, properly means embalmer, a distinct and low caste of persons, who were exclusively employed in offices relating to the dead ; a kind of undertakers ; and so in fact they are designated in the Septuagint, the word *ἐνταφίσται* never being applied to physicians, but as its derivation implies (*ταφή*, burial,) to persons employed about the dead ; such persons we know were held in the utmost abhorrence, for in Egypt, as in other places, those who felt no repugnance to destroying the living, had a pious horror of touching the dead. In Egypt the principle of the subdivision of labour seems to have been carried into medicine to a considerable extent, for according to Herodotus, "every distinct distemper had its own physician, who confined himself to the study of that and of no other, so that all places were crowded with physicians ; one class had the care of the eyes, and another of the head, another of the teeth, another of the stomach, and another of occult diseases." This description, however, cannot refer to the priest physicians, but to the *Iatraliptæ*, and other inferior and unauthorized practitioners, of whom Diodorus and Plutarch make mention. This part, at least, of the Egyptian practice of medicine seems to have been handed down to us unimpaired, for we, too, have our ocu-

lists, and our aurists, and our dentists, and (in practice, if not in name) we have our great stomach doctors, and lung doctors, and liver doctors, a proof that the practice has something more substantial than its antiquity to recommend it. As to the medicines employed by the Egyptians, it would appear that they were acquainted with the powers of some valuable remedies, such as squills in dropsy, and iron as a tonic in cachectic diseases.

Of anatomy (the foundation of all medical science) the Egyptians knew absolutely nothing until the age of Plotemy, Soter, or Philadelphus, 280 years before the Christian era, when human bodies were for the first time dissected in the great school of Alexandria.

The Greeks do not seem to have derived their knowledge of medicine from the Egyptians, for we find that they had made considerable proficiency in the art, long before they had any connexion with Egypt. Witness the various allusions to medical and surgical practice in Homer, and the knowledge which he certainly had of the situation of the principal arteries and nerves, and of the most important viscera of the body ; a knowledge which has induced some enthusiastic critics to endow the great poet with a thorough knowledge of anatomy and medicine, as well as of all other arts and sciences. But the truth is, that it was absolutely impossible the Greeks should have acquired any considerable knowledge of human anatomy, for they burned their dead, and collecting the bones which were not consumed by the fire, they enclosed them carefully in urns. To the Greeks, the most sacred of all obligations was to carry to the bosom of their own country, the ashes of their parents and friends who had died in a strange land. A tomb was an object of veneration ; to disturb the bones, which it was supposed, could only be done for the purpose of employing them in some magic rite, was an inexpressible offence. In war an enemy was but half subdued, if his body was not carried away by the victor ; accordingly, the most desperate combats were fought round the body of the dead or dying chief, lest he should fall into the hands of the enemy, and his body remain unburied, a “prey to the dogs, and the ravening fowls of the air ;” and perhaps the finest and most touching passage in the finest poem in the world, turns on this very feeling of respect for the dead. I doubt if any one can even think without emotion, of the aged Priam’s visit in the dead of night, to the tent of Achilles, to implore him to restore to him the body of his son ; an action which, more than any other in the poem, seems to have awakened the sympathy of all the inhabitants of Olympus.

All these usages were so many invincible obstacles to the

study of human anatomy, whether in towns or in armies, but the greatest of all was superstition. To the fear of interrogating a dead body, was added the guilt and the punishment attached to witchcraft, and we find that, even to a philosopher so enlightened as Cicero, the study of human anatomy seemed to be as revolting as it was unnecessary. "The public," he says, "have a notion, that a knowledge of the internal structure of a man is useful to the physician, and hence it is, that the physicians *pretend* to possess it. In this state of public opinion, physicians and philosophers sought to obtain from the dissection of animals, the information which they could not obtain from human anatomy. "*Tantum enim veteres non modo medici verum philosophi quoque anatomia studuerunt.*" This expression of Galen's must, however, be understood as applying exclusively to the anatomy of animals; and to say the truth, had even this kind of anatomy been cultivated diligently in a truly philosophical spirit, the healing art would not have been encumbered with the trappings of a speculative and vain philosophy by the dogmatics, or degraded into a collection of "family receipts" by the empirics. It is certain, however, even the degree of knowledge which might have been acquired by the dissection of animals, had made but little progress among the Greeks previously to the age of Aristotle, as will appear when I come to speak of the anatomical knowledge of Hippocrates and his successors.

Of the state of medicine in Greece before the era of Hippocrates, we know but little, but that little is not devoid of interest. In Greece, as in Egypt, the chief practice was in the hands of the priests, and the temples of Esculapius were so many great hospitals, or *Maisons de Sainte*, where they carried on their operations. All who had the means of repairing to those temples were accommodated within its precincts: having in the first place laid their offerings on the altar, and been purified by the lustral water, they were put to rest in the middle of the temple, and as soon as they were supposed to be asleep, a priest clad in the vestments of Esculapius, and surrounded by a troop of damsels, who were devoted to the service of the temple, and were well instructed actresses, entered the sacred dormitory, for the purpose of indicating to the patient the remedy which the oracle of the deity had revealed as most suitable to his case. As the god could only reveal himself in a dream, the patients were placed on beds of ram's skins, which were supposed to cause divine visions, and it was considered an impiety for the patient to open his eyes, and not to feign a profound sleep, even though wide awake; and above all, to doubt that

whatever he *did* hear with his ears, or see with his eyes, was other than a celestial vision.

The servant, into whose mouth Aristophanes puts this recital, describes, in a comical manner, the address and promptitude displayed by the chief priest in putting into his sack, while these ceremonies were being performed, all the votive offerings which had been laid upon the altar.

In the time of Lucian these juggleries had fallen into contempt among all educated persons, it therefore became more safe to expose them publicly to ridicule ; he tells us that, even in his time, a priest* established himself in an ancient temple of Esculapius, and found means to prey upon the credulity of the people, so as to amass a considerable fortune, and that he was able to count among his patients some old and foolish Roman senators.

These revelations respecting diseases and their remedies, announced by a priestess, in whom real or pretended madness was an essential qualification for the office, will remind us of proceedings which do not date quite so far back as the days of Pericles, and are not quite so far from home as Greece.

Amongst the number of temples consecrated to Esculapius, were those of Epidaurus, of Pergamus, of Cos, and of Cnidos. The walls and columns of these temples were covered with inscriptions, which recounted, briefly, the histories of the most remarkable diseases, and of the remedies which had been employed, with success, by the power and under the very eyes of the god. The rich had the histories of their cures engraved on plates of brass, or tablets of marble, the poor, on simple blocks of wood. “ It is reported,” says Strabo, “ that Hippocrates, one of the most illustrious personages of the island of Cos, learned the practice of medicine from the sacred tablets which *still* exist in the temple of Esculapius, that stands in the suburbs of the town.”

An ancient tablet of this kind is preserved by the family of Maphæi; the inscription is in Greek. I give a translation of one or two of the shortest of the cases, which will serve to shew the character of them all.

1st case. The god pronounced the following oracle to Caius, who was blind, that he should approach the sacred altar, and having bent his knees, he should pass from the right to the left; after this, he should place his five fingers on the altar, then raising his hand he should apply it to his eyes, which, no sooner had he

* See the Alexander of Lucian.

done, than he saw perfectly well ; all the people present testified their joy that so great a miracle should have been performed in honour of our emperor Antoninus.

2nd. The god gave this oracle to a blind soldier, named Valerius Aper, that he should take the blood of a white cock, and mixing it with honey, use it as a collyrium ; this he did for three days, and his sight was restored ; he came to return thanks publicly to the god, and place this tablet in his temple.

A third is cured of a spitting of blood by eating apples, taken from the altar and stewed in honey.

To the temple of Cos, such as it was, however, we are indebted for Hippocrates, for there it was that he received his first lessons in philosophy and medicine—lessons which, he tells us, he imbibed with his language ; for the ancestors of Hippocrates had, for seventeen generations, exercised the healing art, in all its branches, in the temple of Esculapius.

After what has been stated, with respect to the modes of practice in these temples, it may seem strange that such a physician as Hippocrates could have been formed in such a school. But without referring to the expansive power of genius, which seems to acquire a strength by compression, that enables it to overcome every obstacle that can be opposed to its free course, I should say, that the position of Hippocrates was, of all others, that which was the most suitable to form a great physician : acquiring the first elements of medicine from the mouth of his parents, at the same time that he learned their language ; living, as it were, in a great hospital, he must have gained that habitual knowledge of the aspect of disease which is incommunicable by teaching, and he must have become familiar with the mode of preparing and exhibiting remedies, and of observing their effects. The enduring—the ineffaceable impressions, which are produced by objects which are calculated to excite emotion, and which are presented to us in early infancy, is a matter of daily observation—the first comparisons which are drawn between those objects by the growing reason, are as durable as the impressions themselves ; it is at this period of life that the “education of circumstances” properly begins, and that the character and talents of the individual, though stamped in his organization, acquire those modifications which are to determine the place that he is to hold in society. A careful consideration will convince us, that all our false and vague ideas are given to us by tuition—by our endeavouring to form, from *description*, a notion of things which we have never *seen*, while the ideas which we acquire by our personal observation, and the judgments which we form of them, are usually sound ; but these judgments, to be sound, must be

formed on clear and complete impressions, and the organs destined to receive them must be educated by well directed exercise.

In a word, the objects which Nature presents to us are our true masters, and her lessons have this advantage over the lessons which we receive from men and books, that they are always exactly proportioned to our faculties. One man will observe more or less, better or worse, than another; but no man will observe and retain the impression of *what is not*; or what comes to the same thing, what he cannot entirely comprehend. This is the great advantage of supplying the mind in early life with images which are to form materials for our after judgments; an advantage which Hippocrates had in the Temple of Esculapius; an advantage which every young man who has resided within the walls of an hospital is able to appreciate.

An examination of the doctrine and practice of Hippocrates would occupy many hours, and unquestionably time could scarcely be employed to so little purpose; for the doctrine is visionary, and the practice obsolete. To those who have never heard the name of Hippocrates mentioned, but with expressions of admiration and respect, who have considered him not only as the “*facile princeps*,” but as the very father of medicine; to such (and they are perhaps the majority of those whom I have the honour to address) it must seem little short of profanation to deal so lightly with a name so venerated. I feel bound, therefore, to state the grounds on which I have made a statement so little in accordance with the generally received belief. It will, I think, be conceded, that in any department of natural knowledge, a doctrine which is worthy of the least respect, must have some foundation in nature. A mere speculation, however ingenious, if it be not only unsupported by facts, but in direct opposition to them, cannot be received as a doctrine, and any inferences which may be drawn from such a speculation, must be as false as the foundation on which they rest.

Now anatomy, which relates to the structure of animal bodies, physiology, which relates to the functions which that structure performs; and pathology, which relates to the changes which are produced by disease in the structure and in the functions, all these, which form the foundation of the medical art, were absolutely unknown to Hippocrates; he has written nothing upon any of these subjects, it is only in the works falsely attributed to him that we find some anatomical details, which are at once contradictory and erroneous.

The book on Prognostics, the treatise on Humours, the

celebrated one on the Effects of Air, of Waters, and of Places, that on Regimen, on Fractures and Luxations, on Wounds of the Head, the Aphorisms, the second book of the Predictions, and the first and third on Epidemic Diseases, of which he is the undoubted author, contain nothing absolutely upon anatomy, general or descriptive.

Then for the physiology of Hippocrates.—He considered the functions of the body as under the direction of an intelligent and instinctive principle, which he called *φύσις* (nature); to this principle he committed the distribution of the blood, spirits, and heat to the different parts of the body, which receive by this means life and feeling; “this faculty,” he says, “nourishes, preserves, and causes the growth or diminution of all the parts;” it attracts similar and repels dissimilar parts, preserving only what is useful, and rejecting what is injurious or superfluous. This doctrine has, under various modifications, formed the basis of all the (so called) medical systems which have been broached since his time. The phusis of Hippocrates is the “archæus” of Van Helmont, the “anima” of Stahl, “the vital principle,” or “LIFE,” of Hunter, and the “vis medicatrix naturæ” of Cullen. Hippocrates had no suspicion that the brain was the organ of sensation and motion; he considered it, on the contrary, to be a gland or sort of great sponge, for the purpose of absorbing the pituita and the superfluous humours of the body, and discharging them through the nose. The heart he considered as the origin of the arteries and nerves, (mistaking the *cordæ tendinæ* for nerves); and the liver as the origin of the veins. The lungs he considered as the absorbers and condensers of the humidity of the body. But this is, I trust, a sufficient sample of the state of anatomy and physiology in the time of Hippocrates; with respect to his practice it may be enough to say, that it was founded on hypothetical notions respecting the four humours (as he calls them) of the body—blood, pituita, yellow bile, and black bile. A state of health he considered to consist in the happy proportion in which these humours were combined; and disease, in the derangement of these proportions; then, much depended on the influence of the stars, and on the power of numbers. Seven days were allowed for the acute stage of fever, and seven for the chronic; during the first seven, little or no nourishment was to be given, but during the last, the coction of the humours being completed, the patient was to be amply supplied with food. Among the medical agents which he employed may be counted, blood-letting, local and general, purging, sweating by vapour baths, and some narcotic, which is

suspected to have been opium, but upon this point the learned are still divided. On the whole, however, it may be said that the practice of Hippocrates was, what is called in France, *expectante*. He strongly inculcates the advantage of observing the course which nature is pursuing for her relief, and of removing all the obstacles which may be opposed to her efforts by internal and external influences; but he as strongly protests against interfering with the efforts of nature, by administering powerful drugs, particularly at a time when the coction of the humours is not completed. If this be a just representation of the opinions and practice of Hippocrates, what, it may be asked, are his claims, to the admiration and respect amounting almost to divine honours, which have been attached to his name for upwards of 2000 years?

The answer is easy; so long as medicine was a speculative science, the speculations of Hippocrates (conveyed as they were in a tone of authority, with all the advantage which the most consummate genius can impart to its subject) were not only as good, but far better than any others. Then his descriptions of the signs of disease, and his prognostics as to life or death, drawn from an exact observation of those signs, are without a parallel. These were strong claims, but perhaps the strongest was the opinion transmitted from age to age, and received without examination, that he was the parent of the healing art, and that from his decisions there could be no appeal.

But what, it may well be asked, has produced such a change in the state of opinion respecting the merits of Hippocrates?

The answer is to be found in the museum of this college, and in the clinical wards of our hospitals. *Observation* and *experiment* have taken the place of speculative and of misapplied philosophy, and medical philosophers are now content to inquire *what* is done in the animal economy, and not *how* it is done.

One hundred and thirty years after the death of Hippocrates, and one hundred and eighty before the Christian æra, Herophilus and Erasistratus, under the protection of Ptolemy Soter and Philadelphus, who were the great patrons of the sciences and the fine arts in Egypt, dissected human bodies in the school of medicine, which those enlightened monarchs established at Alexandria; the works of these anatomists have not reached us, but we learn from Galen several particulars respecting their discoveries.

Erasistratus discovered the *lacteal vessels* on the mesentery of a kid that had drunk milk shortly before it was killed; he was ignorant, however, of their functions. He also discovered and named the valves of the heart, and traced the origin of the

arteries and veins to this organ. According to him, the air passes from the lungs to the heart, which performs the functions of a smith's bellows, attracting the air by the dilatation of the left auricle; from the left auricle it passes by the arteries which contain air, or rather animal spirits, to every part of the body. The veins contain all the blood, and according to this supposition, fever and inflammation are the consequence of any portion of blood passing, by an *error loci*, from the veins into the arteries.

Here is the germ of Boerhave's doctrine of inflammation being caused by the passage of red blood into the capillaries, which should contain (certainly not air) but the colourless and aqueous parts of the blood. Herophilus, whom Galen describes as a man consummate in every thing relating to medicine, also studied anatomy in the human subject, but his works have not come down to us. It is stated by Celsus* and Tertullian,† that Herophilus and Erasistratus committed the horrible and indeed incredible cruelty of dissecting criminals alive, who had been handed over to them for that purpose by the Ptolemies; it is infinitely more probable, that so unusual an occurrence as the dissection of a dead body, and one so calculated to outrage public feeling, should have been exaggerated into the dissection of the living.

Nothing worthy of notice is to be found in the history of medicine from the time of Herophilus and Erasistratus, at whose death, it would appear, that the practice of dissecting human bodies was discontinued, until the age of Galen, whose genius and labours gave a new character to the art. I pass over Celsus, because although an elegant and classical writer, he is but a compiler, and it has even been disputed that he was a medical practitioner, but it is certain that his knowledge of anatomy and physiology, extended no further than that of Hippocrates, from whose works he chiefly compiled his own. Quintilian, in the twelfth book of his *Institutions*, has this remarkable passage respecting Celsus, speaking of how much can be done by diligence, he says, “*Quid plura? cum etiam Cornelius Celsus, mediocri vir ingenio, non solum de his omnibus conscripserit artibus, sed amplius rei militares et rustica etiam et medicina precepta reliquerit, dignus vel ipso proposito ut eum scisse omnia illa credamus.*” A learned and ingenious commentator, and an enthusiastic admirer of Celsus, has suggested that the “*vir mediocri ingenio*” was a mistake of the copy-

* Celsus, *Præf.* lib. 1.

† Tertullianus de Anima.

ist, and that it should be read *medicus acri*, instead of *mediocri*. This is, I am afraid, somewhat in the manner of the emendations of Martinus Scriblerus, who suggests, that the “*volvitur caput*,” as applied to Palinurus, when he fell overboard, should be *volvit, ter, caput*; but however this may be, I scarcely think a man *mediocri ingenio*, could write the beautiful passage which I shall now read, of which it would be difficult to say whether it is most to be admired for the justness of the precept, or the classical purity of the expression; it will at all events obtain the approbation of those who think with Rochefoucault, that too much attention to regimen makes life but a long disease.*

Next comes Galen, but this is a large subject, five folio volumes of his works have descended to us, and these do not contain the one-half of his labours. Galen was a man of considerable acuteness, of great learning, and of incredible industry; his industry, however, turned in a great measure upon matters of but little utility, it was wasted on voluminous commentaries on Hippocrates, and on speculations respecting the functions of the animal economy, of which he knew but little, either from observation or experiment. Some notion of the state of experimental philosophy in the time of Galen, may be formed from the account which he gives of the experiment by which he convinced the assembled physicians and philosophers of Rome, that air was contained within the cavity of the chest, between the lungs and the pleura costalis; he says he explained to them the manner in which the air passed from the lungs through the cribriform plate of the æthmoid bone into the ventricles of the brain, in which a true respiration was performed, the organ rising and falling in correspondence with the motions of the chest, and the air escaping through the sutures and the palate.† His description, however, of the various

* “*Sanus homo, quid bene valet, et suæ spontis est nullis obligare se legibus debet, ac neque medico, neque iatrolepta egere; hunc oportet varium habere vitæ genus: modò ruri esse, modò in urbe sæpiusque in agro: navigare, venari: quiescere interdum, sed frequentius se exercere, si quidem ignavia corpus hebetat, labor firmat: illa maturem senectutem, hic longam adolescentiam reddit. Prodest etiam interdum balneo, interdum aquis frigidis uti: modò ungi, modò id ipsum negligere: nullum cibi genus fugere quo populus utatur: interdum in convivio esse, interdum ab eo se retrahere: modò plus justo, modò non amplius assumere: bis die potius quam semel cibum capere, et semper quam plurimum dum modò hunc concoquat. Sed ut hujus generis exercitationis cibique necessarii sunt, sic athletici, supervacui. Nam et intermissus propter civiles aliqua necessitates ordo exercitationis, corpus affligit; et ea corpora quæ more eorum repleta sunt, celerrime et senescunt et ægrotant.*”

† *De Usu Partum*, lib. 8, cap. 10.

parts of animals, which he had certainly dissected with great care, are original; but his speculations on the uses of those parts, are very rarely correct.

Galen was born in Pergamus, in the second century of the Christian era. He tells us that he studied philosophy in the schools of the Stoics, the Epicureans, the Academicans, and the Peripatetics: having resided for some years at Alexandria, he returned to Pergamus, where the pontifex charged him with the care of the wounded gladiators; in his thirty-second year he visited Rome, and gave anatomical demonstrations in the presence of the consuls Severus and Boetius, Sergius Paulus, the chief governor of Rome, and the uncle to the Emperor Lucius Verus, Endemius the philosopher, Adrien the rhetorician, and several others of the most distinguished personages in Rome. It would appear then that in the best days of the empire, researches connected with the advancement of the healing art, were then, as now, thought worthy of the attention of the persons the most illustrious by their rank and talents in the greatest nation of the world. Galen unquestionably made many important additions to anatomical knowledge; he gives an excellent description of the brain and the organs of voice and respiration, and is the first to mention cataract, and the operation of couching for its removal; in his ignorance of human anatomy, however, he hazards rather an infelicitous speculation. "If," says he, "the opacity be in the film or membrane which is behind the pupil, it may be removed by tearing it up with a needle; but if it should be in the crystalline lens, then nothing can be done, for that is the essential organ of sight." It is a remarkable and instructive instance of the difficulty of getting rid of an error which has the support of an accredited name, that it was received as an article of faith until the middle of the last century, that in the operation of couching, it was a pellicle and not the crystalline lens that was removed from the axis of vision. But I must leave Galen, or I shall not be able to touch upon the subject in which we must all take a stronger interest, I mean the discoveries in the healing art, which have illustrated the age in which we live, and in the benefits of which we all participate. I pass over the Arabians, they were but the copyists of the Greeks, and though we are mainly indebted to them for preserving what remains to us of the literature and science of antiquity, they have made no additions to the healing art. Like the Greeks and the Romans, they knew nothing of anatomy or the dissection of dead bodies, as the merely touching them was considered as an impurity which was to be expiated by an infinity of ablutions and other most

troublesome observances. I may just observe in passing, however, that we are indebted to Rhases, an Arabian author in the seventh century, for the first exact account of small-pox and measles, two diseases which made their first appearance in Europe in the course of that century.

Pressed as I am for time, I cannot resist reading you one short extract from Rhases, an Arabian writer whom I have just mentioned, which will serve to show how besetting a sin quackery is, how deeply it is engrained in human nature, and how exactly quacks, in all times and in all nations, proceed upon the same principles, and even adopt the same practices.*

At length we come to the sixteenth century, that glorious æra for the human race, when the mind of man, starting up from the sleep of seven centuries, (a sleep that looked like the sleep of death,) rushed forward with all the force, but unhappily, too, with all the intoxication of "a giant refreshed with wine."

All the sciences and all the arts felt the beneficent influence of this mighty revolution in the human mind, but none so much as the healing art. For 1500 years, that is to say from the time of Herophilus and Erasistratus, to the reign of the Emperor Frederick the Second, anatomy was a lost art; during 800 years of that period, medicine was handed over to philosophy, was converted into a speculative science, and consequently made no progress as a useful art. During the remaining 700 years, it shared the fate of every other department of human knowledge, and was made up of superstitious rites, amulets and charms, and of terrible and irrational operations.† At length,

* "There are so many little arts used by mountebanks and pretenders to physic, that an entire treatise, had I a mind to write one, would not contain them. Some give out that they can draw snakes or lizards out of their patients' noses, which they seem to perform by putting up a pointed iron probe with which they wound the nostril till the blood comes, then they draw out the little artificial animal composed of the coagulated blood, or of liver, &c. Some pretend *that they can collect all the floating humours of the body to one place by rubbing it with wild cherries, which causes a burning or inflammation*, and they expect to be rewarded as if they had cured the distemper." "Had Rhases lived in our days," (adds Dr. Friend,) "he might have found subjects who would have well resembled the picture he has here drawn."

† The state of surgery in his time, is thus described by Guido de Chauliac, who wrote about the middle of the fourteenth century. "Practitioners are divided in five sects: the first follow Roger and Roland and the Four Masters, and apply poultices to all wounds; the second follow Brunus and Theodoric, and in the same cases use only wine; the third follow Saliceto and Lanfrane, and treat all wounds with ointments; the fourth are the Germans, who attend the armies, and promiscuously use charms, potions, oil, and wool; the fifth are those who have recourse to the saints in all cases." To these classes seemed to have belonged the

however, came the celebrated ordonnance of the Emperor Frederick the Second, son of Henry the Sixth, one of the articles of which runs thus: "Salubri etiam constitutione sancimus ut nullus chirurgicus ad practicam admittatur, nisi testimoniales litteras afferat magistrorum in medicinali facultate legentium quod per annum saltem in ea parte medicinæ studuerit, quæ chirurgiæ instruit facultatem et præsertim *anatomiam humanorum corporum in scholis didicerit*, et sit in ea parte medicinæ perfectus, sine qua nec incisiones salubriter fieri poterunt nec factæ curari." Mundinus, an Italian physician, under the protection of this ordonnance, anatomized the bodies of three women in 1315, and gave the result of his dissection in the first treatise of human anatomy which had been published since the days of Hieropholus, a period of 1500 years. It appears too, that about this time coloured anatomical plates were employed in demonstrating the different parts of the body, by Henry Hermundaville. The physicians of Montpellier, instructed by the Jews and the Arabians, obtained permission in 1376 from the Duke of Anjou, Governor of Languedoc, to take each year the dead body of a criminal that had been executed. This permission was afterwards ratified successively, in 1396 by Charles the Sixth, and in 1496, by Charles the Eighth. Gui de Chauliac, who published the first system of surgery that ever appeared in France, wrote his great work about this time; and there can, I imagine, be little doubt that the appearance of the work may be attributed to the encouragement which was given to anatomy during this age. He speaks with great approbation of Master Barthelemy, who taught anatomy on a human body in Montpellier. In 1494, the physicians and surgeons of Paris attempted, with but little success, to introduce human anatomy into the schools of medicine, as it was discouraged by the ecclesiastics. In Italy superstition made new efforts to arrest the progress of anatomy; for in 1572 a physician of Parma was forbidden to teach it under pain of excommunication.*

surgeons introduced by Chaucer in the Knight's Tale, as attendants on the tournament.

"Men say den eke that Ascite shal not die,
He shal ben healed of his maladie.
All were they sorely hurt, and namely one
That with a spear was thirld his breast bone.
To other woundes, and to broken arms,
Some hadden salves, and some hadden charms,
And fermacies of herbes and eke save
The drunken; for they would their lives have."

But this relates only to their practice in trifling matters, in the more important recourse was had to the most horrible cuttings and burnings.—See *J. Bell's Surgery, passim*.

* See Guerneri Rolimii *Dissertationes Anatomicæ*, Noribergæ, 1656. p. 187.—

In 1556, however, anatomy was firmly established in the schools by Charles the Fifth, who proposed it as a question to the theologians of Salamanca, "whether it was permitted to Catholics to open human bodies?" The Spanish doctors answered that it was useful, and therefore lawful, and supported their opinion by a reference to the permission given by the Ptolemies to dissect, for the benefit of living, the bodies of dead criminals.

A few years before this, appeared the great work of Vesalius, which I cannot but think is one of the most astonishing monuments of genius and industry that has ever been produced. Here is the work, which he completed in the 27th year of his age, and printed in the year 1543. When we consider that when Vesalius commenced his labours, there was nothing extant on the subject of anatomy, but the wretched treatises of Galen, and the small work of Mundinus, giving the result of the dissections of three bodies, with what astonishment must we view a work so complete, that little of the anatomy of the human body, and that little absolutely unimportant, has been left unobserved.

I wish I could add that such genius and industry had met with a suitable reward; but unhappily for Vesalius, like most others of the greatest benefactors of mankind, he was born in an age to which he did not naturally belong; persecuted by envy during his life, pursued even to death by ruthless superstition, he died of hunger on a desert island in the Egean Sea, to which he had been banished by a decree of the Inquisition, on a charge of impiety, for having opened a woman while she was yet alive. His cruel fate has been so simply and touchingly described by his cotemporary Ambrose Paré, Surgeon to Charles the Ninth, that I am tempted to read it.

"Il ne faut point se hâter d'ensevelir et encore moins d'ouvrir le corps des femmes hysteriques de peur d'encourir une calomnie ainsi que de ce siècle est arrivé à un grand anatomiste; je dis grand et célèbre duquel les livres réparent aujourd'hui les études des hommes doctes, lequel étant pour lors residant en Espagne fut mandé pour une femme de maison qu'on estimoit être morte par une suffocation de matrice. Le deuxième coup de rasoir qu'il lui donna commença ladite-femme à se mouvoir et à démontrer par autres signes quelle

"Nicholao Bucello Medico Patavino anno 1571 privatas anatomias institueret, interdictum id ipsi sub gravi pœna fuit; excommunicationes nempe quas jus canonicum minatur illis qui privatim incidunt humata cadavera respexerunt ad, Bonifacii VIII. hujus nominis Pontificis maximi, qua vetebatur evisceratio et cadaverum in aquâ decoctio ut ossa à tegumento carnis nudata in patrias terras ad sepulturam deveherentur, constitutionem."

vivoit encore dont tous les assistans furent grandement étonnés. Je laisse a penser au Lecteur, comme ce bon seigneur faisant cette œvres fut en perplexété comme on cria *Tolle* apris lui, tellement que tout ce qu'il put faire fut de s'absenter du pays, car ceux qui le devoient excuser c'étoient ceux qui lui couroient sus, et étant exilé, tôt après mounet de déplaisir, qui n'a été sans une grande perte pour la republique."

The affair, however, had a different issue from that related by Paré. It appears, that pursued by the relations of the woman according to some, or of a nobleman (according to others,) whose trance he mistook for death, (although it is highly probable, that the story was got up for his destruction,) he was condemned to death by the tribunal of the Inquisition, but at the interposition of Philip II., whose physician he was, and by whom he was beloved, sentence of death was commuted to a pilgrimage to the Holy Land. Returning from Jerusalem, his vessel was wrecked on the Island of Zante, near the Morea, where he died of cold and hunger, on the 15th October, 1564.*

The revival of anatomy in the sixteenth century, was soon signalized by the discovery of the circulation of the blood, by the immortal Harvey, a discovery, the importance of which need not be discussed in the assembly which I have the honour to address. It should not be concealed, however, that Harvey was greatly assisted by the previous discovery of Servetus, that the blood did not (as Galen taught, and as was universally believed) pass from the right to the left ventricle, through the septum that divides these cavities, but that it passed from the right ventricle of the heart by the pulmonary artery to the lungs, and returned to the left ventricle by the pulmonary veins; the communication between the arteries and veins taking place in the substance of the lungs; this beautiful discovery lay concealed for many years, in the rubbish of a theological work, entitled *Christianissima Restituta*, printed in 1553. Here is the tract, which has been preserved in the fourth volume of Lord Somers' Tracts.

The circulation of the blood through the lungs once established, the circulation through the system at large followed of course, and there can, I think, be no room to doubt, that had Servetus followed up his discoveries, he would have arrived at the result which Harvey so happily came to nearly a century afterwards. Servetus, however, instead of following

* See the life of Vesalius in the Preface to his works, edited by Albinus, 1725, and Dictionnaire Universelle, Art. Vesalius.

up his anatomy, took to theology, and was, at the instigation of Calvin, burned to death, together with his book *De Erroribus Trinitatis*.

The discoveries which followed, and “trod (as it were) on the others’ heels,” are so numerous and important, that I feel considerable difficulty as to the course which I should adopt, in bringing them under your notice. I believe the least objectionable plan will be, to refer to a *few instances*, in which the improved spirit in which anatomy has been cultivated of late years, has led to some interesting or useful discoveries. I shall observe no chronological order in these remarks, but follow, for convenience sake, the arrangement suggested by the usual anatomical division of the human body into head, trunk, and extremities. First, then considering the exceeding difficulty of the investigation, great praise is due to Gall,[†] for the discovery of the decussation of the medullary fibres of the upper portion of the spinal cord, before they go to form the corpora pyramidalia. He has traced them across the pons varolii, the thalami, and the corpora striata, up to the vault of the hemispheres; many nerves, which were supposed to proceed immediately from the brain, have been traced by him into the medulla oblongata, where, he thinks, they decussate, before they go to form the corpora pyramidalia.

Such a decussation had been inferred by Cassius, surnamed the Medical Philosopher,* who lived about a century after Hippocrates: he asks in one of his problems, which are still extant, why when a wound is inflicted on the right side of the head, is the left side of the body paralysed? and he answers, “it depends on this, that the nerves which come from the base of the brain, cross one another, those of the right side going to the left, and those of the left to the right.” The observation of the fact is as old as Hippocrates, but Cassius is the only writer who offered an explanation, which has since been verified by the researches of Gall.

The division of the nerves into nerves of motion and nerves of sensation, and their origin by distinct roots, proceeding from opposite sides of the spinal cord, is a most beautiful and important discovery, for which we are indebted to Sir Charles Bell. This discovery, I need not say, serves to explain some of the most obscure, and hitherto inexplicable phenomena of life, and must exercise no inconsiderable influence on the healing art.

Cuvier, in treating of the discoveries in anatomy which

* Tatrosophista, p. 226.

have been made within the last century, says of these discoveries : “ the greater number relate to animals, but man has furnished one, although we could scarcely expect it, after the unremitting researches which have been carried on during more than three centuries. Mr. Soëmering has had the good fortune to find in the centre of the retina of the human eye, a yellow spot, a projecting fold, and a transparent point, which had escaped the observation of all his predecessors ; we are still ignorant of the use of it, but it is remarkable, that it is to be found only in man and in quadrumanous animals.” Our learned and ingenious Professor of Anatomy, Dr. Jacob, has been no less fortunate, he has discovered in the human eye, a membrane of the most exquisitely delicate texture, which covers the internal surface of the retina, that is to say, the surface in contact with the vitrious humour, and is connected with it by cellular texture and vessels. An account of this discovery is published in the Philosophical Transactions for the year 1819. Your reporter has been equally fortunate in discovering in the eyes of birds, a distinct muscle arising from the inner surface of the bony hoop which surrounds the cornea, and terminating in a circular tendon, which is connected with the internal lamina of the cornea. I was led to the discovery from the consideration, that the faculty of adapting the refractive power of the eye to the different distances of objects, must exist in a higher degree in birds than in other animals. An eye, with a high degree of refractive power, is well adapted to the uses of the animal while it rests upon the earth, but when it soars in the middle region of the air, the rays proceeding from the objects below must arrive at the eye in lines, which may be considered as parallel, consequently, to form anything like a distinct image, this refractive power must be lessened as the divergency of the rays decreases.

It occurred to me then, that if this change in the refractive power of the eye were effected by any mechanical contrivance, that contrivance would be, in all probability, more conspicuous in birds than in other animals.

It was, therefore, with no common feelings of satisfaction, that I found in the eye of the ostrich and the eagle, and afterwards in all other birds, the mechanism for which I looked, and which is displaced in this preparation.

The discovery of the third lobe of the prostate gland by Sir E. Home, has thrown considerable light on the nature and treatment of certain diseases of the urinary organs.

The discovery of auscultation, whether mediate or immediate, by the illustrious Laennec, has almost realised the wish of old Mayow, that “ some one would place a window in the chest,

through which we might be able to discern all that is passing within its walls." Great discoveries have accordingly been made, and are daily making, in the pathology of the thoracic viscera. Nor have the pathologists in this country been idle in following out the inquiries which originated with Laennec, they have, on the contrary, made ample and most valuable contributions to this important branch of medical science.

Auscultation has also been applied with the greatest advantage in detecting the early stages of pregnancy, two distinct sounds—the sounds of the foetal heart, and the placental murmur—being distinctly discernible through the parietes of the abdomen of the mother.

Dr. William Stokes's work on *Diseases of the Chest*, besides being the fullest and ablest treatise on the subject which has as yet appeared, contains several original observations of the utmost value concerning the diagnosis of thoracic aneurisms. Dr. Corrigan has had the good fortune to discern the signs by which a permanently patulous state of the aortic valves may be detected, thus clearly distinguishing a disease, the symptoms of which had hitherto escaped the observation of all pathologists; he has also established the diagnosis between phthisis and a peculiar disease of the lungs, which he was the first to observe and to describe, discoveries which must rank Dr. Corrigan among the ablest pathologists of the present day.

Every discovery in anatomy, whether normal or morbid, which leads to an improved practice, tends to elevate medicine from the rank of an empirical art, to that of a science; for science is well defined to be "an art built upon principles." The observation, therefore, that certain forms of jaundice are dependant on an inflamed state of the mucous membrane of the duodenum, is a valuable addition to medical science. For this important observation we are indebted to Dr. Marsh, whose paper on the subject is contained in the 3rd volume of the *Dublin Hospital Reports*. The unequalled opportunities which this city affords of advancing obstetrical knowledge, has been turned to good account; I do not allude to the prolific powers of its inhabitants, but to the magnificent hospital for lying-in women, the largest and best arranged in Europe; these opportunities have enabled the late Master, Dr. Collins, to bring forward a work, replete with the most valuable information, both practical and statistical. The present Master, following the steps of his predecessor, with no less ability, has greatly extended the benefits of the hospital by opening a ward for the treatment of the diseases of women. A paper by Dr. Evory Kennedy, in a late number of the *Dublin Medical and Surgical Journal*, furnishes ample proof that the

opportunity he has thus created of investigating a most difficult and interesting class of diseases will be turned to good account. While on the subject of obstetric medicine, I cannot but mention in terms of the highest commendation, the obstetrical museum of Sir Patrick Dun's Hospital, created by the personal exertions and skill of Dr. Montgomery, the distinguished Professor of Midwifery at that institution.

If there be any operation in surgery which more than another has felt the beneficent influence of the improved state of anatomical knowledge, it is the operation for strangulated hernia; I will not say that we are indebted to Sir Astley Cooper for the discovery of the true anatomy of the parts concerned in hernia, but I can say with perfect truth, that he is entitled to all the honour of the discovery, for he was the first to apply knowledge, which was of no account until he used it in explaining the nature and treatment of strangulated hernia.

This part of surgery seems in the present day to have received all the improvement of which it is susceptible, and for this great gift to humanity, I rejoice to say we are indebted to one of the ablest surgeons and most estimable men that England or any other country has ever produced. Mr. Colles, following the same line of investigation, has, in his valuable treatise on the Surgical Anatomy of the Pelvis, made some original and valuable contributions to the pathology of hernia.

Much light has been thrown on the surgical pathology of the larynx and trachea, and on the operation of bronchotomy, by our learned President, Mr. Porter, in a work which is in the hands of every surgeon in Europe. The discovery of the third insertion of the external oblique muscle by M. Gimbernaut, has led to the complete understanding of the pathology of femoral hernia, and a subject until then but little, if at all, understood.

I have reserved for the last, the greatest triumph which surgery has achieved, in our own or perhaps in any time, I mean the operation of lithotrity, an operation by which, in by far the greater number of cases, a stone may be removed from the bladder with less pain or danger than is attendant upon any surgical operation that I know of. The first hint of this operation is supposed to have been given by Colonel Martin, who in the year 1800 constructed an ingenious instrument, by which he attempted to reduce the stone to powder within the bladder. The instrument consisted of a file enclosed in a canula, the file being worked by means of an elastic stillette. This contrivance of Colonel Martin's in no degree detracts from

the merit of M. Civiale, who was beyond all doubt the first to invent and successfully employ an instrument by which he reduced a calculus to powder, and thus procured its discharge from the bladder.

The operation has since undergone many modifications, in the hands of M. Civiale and others; but the greatest improvement, and that which has altogether superseded the original operation of M. Civiale, is the introduction of the two-branch instrument by Mr. Weiss or Mr. Heurteloup, or rather by both; for I believe each was ignorant of the invention of the other. M. L'Estrange is entitled to the praise of having adapted a screw to the moveable branch of the instrument, by means of which, in a great majority of instances, the calculus may be reduced to powder without the use of percussion. It is true, that about the same time the same contrivance was suggested by Mr. Weiss of London, but I am quite certain that neither of the gentlemen borrowed the idea from the other. Here is the instrument as it is now employed all over Europe; I have suggested a slight alteration, which is I believe generally adopted, it removes the only (and it was rather a serious) objection to the two-branch instrument. The detritus of the stone, instead of being lodged between the jaws of the instrument when they are approximated, in the improved instrument escapes through the slit in the fixed branch. I am indebted to Mr. Oldham, the ingenious engineer for the Bank of Ireland, and now to the Bank of England, for having carried my views into effect, by constructing this beautiful instrument in the manufactory over which he presides in the Bank of Ireland.

A question was raised in the Academy of Medicine in Paris, about three years since, as to the extent to which lithotritry might be considered as superseding lithotomy; the point to be determined was, "whether lithotomy should be the rule or the exception." The matter was discussed with great vehemence during several sittings, but no conclusion was arrived at.

I think I am enabled to state, from personal experience, that lithotritry is to be preferred in the great majority of cases to lithotomy, and upon the following grounds. The only reasonable objections which have been made to lithotritry are:—

1. That it is not applicable to children under fourteen or fifteen years of age, yet a great proportion of those who suffer from calculus in the bladder are under that age.

2. It is not applicable to very large and hard stones, as the operation requires to be so frequently repeated, that it at length excites irritation and inflammation of the bladder, and the frag-

ments which are too large to be discharged, stick fast in the urethra, exciting a train of most painful and dangerous symptoms.

The answer to the first objection is, that the operation *may* be, and, in point of fact *has* been performed by Dr. Smith of Philadelphia, with perfect safety and success on children of three or four years of age, and on one of a year and ten months. The second objection is not so easily disposed of; it is quite true that a very large stone requires repeated operations for its destruction. I have operated in one case as often as fourteen times; but if the operations be carefully conducted, they are unattended by any considerable pain, and are not followed by the slightest constitutional irritation.

The serious and *real* objection is the stopping of large angular pieces in the urethra, such pieces (*e. g.*) as these. Well, I was led by necessity to practise an operation in the case in which these fragments were extracted, which succeeded perfectly, and which at once removes the objection grounded on the stopping of the pieces in the urethra. If the piece lodge, as it almost invariably does, in the bulb of the urethra, near to its anterior extremity, I cut upon it, and remove it with a common dressing forceps. If it should advance farther, I push it backwards to the same situation and then cut upon it; through the opening thus made, I introduce these slightly curved spring forceps, and, seizing the remaining fragments, extract them one by one through the wound in the perineum. By this simple operation, I removed, in the course of a few minutes, all the large fragments which you see in this box. This is not an occasion for entering into a history of the cases in which I have performed the operation of lithotrity; but I may state to this College, that the result of my experience is, that the operation of lithotrity should be the *rule*, and that lithotomy should be the *exception*; and it is my firm conviction, that every year, more and more of the excepted cases will come within the province of lithotrity. Before I conclude this subject, I wish to mention a fact, which I think will be heard with equal surprise and pleasure by this assembly, and that is, that the first operation of lithotrity which perhaps was ever performed, but at all events of which there is any record, was performed here in Dublin, about 270 years ago, on no less a personage than Sir Henry Sidney, the Lord Lieutenant, or rather the Lord Deputy for the time being.

For this most curious piece of information, I am indebted to a gentleman, who naturally enough takes no inconsiderable interest in the subject, since I have within the last twelvemonth performed a similar operation upon himself. This gentleman,

who is one of the most accomplished men of my acquaintance, pointed out to me the passage in Collins's *Lives of the Sidneys*, where it would in all probability have remained buried for 270 years longer, but for his accurate and extensive knowledge of history, and unequalled memory. The paper, in MS., is lodged in Her Majesty's State-paper Office in Ireland, book iii. p. 259, February 1567.

“ *The State of Sir H. Sidney's Bodie, MS., Ireland, 1559, in Her Majesty's Office of Papers and Records of State.*—My Lord President, being of the age of xxxvi. yeares, went into Ireland a hole man, not touched with the stone, and so remained one yeare and a half or thereabouts, and then, after long grief, avoided two stones which were very big, such as few men have been known to have avoided. After this he took his journey into the north parts of Irelande, and so continued void of pain or grief until his arrival in Englande, which was about 8 weeks after, and then at Chester felt the like grief as at first, and so continued in pain until Christmas Eve; at that time being searched with surgeons he avoided one other stone, *broken by the surgeon his instruments in divers pieces*, for that it was so great that otherwise it could not be taken out, for all the pieces laid together might make the quantity of a nutmegge.”—*Collins's Lives and Actions of the Sidneys*, p. 95.

I have yet to mention an improvement in the healing art, which I consider as the most important of all, as being that which has conferred the greatest benefits not only on individuals but on society at large, I mean *the improvement in the education and character of those who exercise the art*.

The truth is, that medicine has been touched by the spirit of the times, which (whatever it may be in other respects) is a humane and rational spirit.

If the wisdom and strength of a government is no longer estimated by the number and severity of its punishments—by the quantity of blood which is shed on the scaffold, so the knowledge and skill of the surgeon is no longer measured by the number and severity of his operations. Operation, on the contrary, is now considered as the opprobrium of surgery, as severity is the opprobrium of a government; for if both were perfect, there would be no need either of operations or of punishments. The diseases of the body, and crimes, (which are but diseases of the mind,) would be prevented or cured by a *rational* treatment, without having recourse to the *ultimum remedium*—the shedding of blood; but although this is a state of things which never can exist so long as man is frail and mortal, still we should for ever strive to approach to that perfection to which we cannot altogether attain; in the conviction that

every step which we make towards it is so much gained for science and for the general interests of humanity. I am fully aware that in the very imperfect sketch which I have ventured to give of the progress of the healing art I have not even completed the outline, far less have I filled up any part of the picture. In the course which I have pursued, I have proposed to myself as a model the conduct of the guides who attend you through a foreign town—they lead you through the principal streets, point out the principal monuments of the arts, but they do not pretend to inform you of what is passing in the houses on either side of the way.

Observations on the Oriental Plague, and on Quarantines, as a Means of arresting its Progress, addressed to the British Association of Science assembled at Newcastle, in August, 1838. By JOHN BOWRING.

WE should not be doing our duty conscientiously to the public, were we to pass in silence the pamphlet bearing the above title, and which, we think, the test of experience will prove to have dangerous tendencies with respect to the welfare of these kingdoms in general.

In limine, it is right to premise, that we look upon this subject with the eye of the physician, Dr. Bowring evidently with that of the merchant ; and, moreover, we are willing to confess, that we are neither prepared nor anxious to enter into a controversy, having for its subject, on the one hand, the mercantile prosperity of “the land we live in,” and on the other, the tenure of human life to be allowed to its inhabitants.

We should be sorry that any uncourteous expression should escape us, yet must we say, that we consider the reasonings brought forward by Dr. Bowring, far from being conclusive in proving the non-contagiousness of plague, and some of them may be taken as no mean evidence of that which they are intended to disprove ; as for instance, page 9, Dr. Bowring says, in endeavouring to prove that the Turks are non-contagionists :

“The theory of the contagionists is this, that the belief in fatalism, the doctrines of irrevocable destiny, are the causes which induce the Mahomedan population to expose themselves unhesitatingly to the perils of the plague. But I never could discover that the doctrines of fatalism led them to subject themselves unnecessarily to other diseases and dangers. I never observed them wanting in prudence to avert, or in sagacity to avoid the ordinary perils of life.”

At the foot of the same page the following note is appended :—

“The firman of the Sultan which establishes a board of health at Constantinople, has the following expressions : ‘ Though to shun the evils with which God visits us would be to pretend to immortality, it is allowed us to fly from the house we inhabit, if that house be threatened by an earthquake ; it is allowed us to pass rapidly by a wall which is about to tumble down ; so ought we to preserve ourselves from the danger of the plague ; for according to the spirit of the noble law, (the Koran,) *the plague is like a fire which destroys all it touches.* ’ ”

And again—

“The body of the Ulemas have proclaimed in council, that as Allah sends evils, so Allah can remove them, and there is nothing opposed to the Divine law in man’s attempts to rid himself of them.”

Can any reasoning or reasonable man read these passages, and draw any other conclusion from them, than that the Mahomedan is both a contagionist and a fatalist. We would be glad to know if Dr. Bowring has ever experienced the horrors of a quarantine lazaret in his own person ; we should suppose that he had, and that smarting and irritated at the delay and expense of those (in the East) ill-managed institutions, he was now breathing forth his wrath against the existence of such institutions at all ; but if it be true that this disease be not dreaded in the East as contagious, a matter we very much doubt as a general proposition, then let the East first give up its quarantine system if it will, let the disbelievers in contagion first make the experiment ; but let it not be said that we have not raised up our voice to decry any proceeding which in the slightest would render the Christian lands to the north of the Mediterranean less safe from the invasion of this most formidable of all diseases.

The subject of contagion is one of immense difficulty, and involving so many considerations, that no one should judge hastily ; and hastily, we conceive, Doctor Bowring has formed his opinion, and most contemptuously has he treated the opinions of those noble and self-devoted men, who since the year 1833, have laboured unremittingly in the cities, hospitals, and dead-rooms, where plague prevailed, in an endeavour to elucidate its actual nature. We refer to page 3, where Dr. Bowring says :—

“I do not mean to refer to the state of that controversy, except to state that a very large portion of the evidence which has been brought forward is of a secondary character ; it is not the evidence of observers, it is not the evidence furnished by those who had seen

or studied the plague in the regions of its greatest ravages, who had watched its origin and progress, or even by those who had the means of sifting the character of the adduced testimony, by any sufficient inquiry, as to the interests, prejudices, and aptitude of the primary witnesses."

O Shades of Reymonnet, Fourcade, Dussap, and Rigaud, ye who have even sacrificed your lives in investigating the nature of this disorder, listen to your traducer confidently accusing ye of ignorance, unfitness, and prejudice! but fear not that your fair fame shall ever be sullied by the coarse breath of calculating calumny; or can the same accusation be brought against your brother survivors, Clot, Aubert, Boyer, Lachese, Gaetani, and others, who are at this moment endangering their lives in this perilous research? Are these men unfitted for making the inquiry? Nor must we omit in this enumeration the spirited Bulard, whose admirable description of plague, with its pathology, is to be found in the 29th Number of this Journal for July, 1838.

Now granting to Dr. Bowring "that a great deal of the evidence floating about in the public mind was of a very untrustworthy character as regarded the contagiousness of plague," still is there not sufficient evidence on every side of a highly respectable character to fully counterbalance any assumption which might exist of its contagiousness by interested persons, whether belonging to boards of health or quarantine stations; or can it be supposed for a moment that those persons would be placed in such boards or stations if not judged fit to fill them by their respective governments; and that no illiberality has been shown by the Pacha of Egypt in the selection of medical officers to fill such important posts, is evidenced by the fact of Clot Bey, a physician whose opinions are known to be sceptical with regard to the contagiousness of plague, being placed at the head of the medical department in Egypt, where his power, influence, and example should in our opinion be more likely to lead those *supposed venal boards* to follow him in adopting non-contagious doctrines, in preference to propagating those of contagion.

Dr. Bowring assumes as one of his strong arguments, the disbelief of the Mussulmen in contagion in plague; but in the next page he states, "that any evidence which comes from the East should be cautiously sifted;" he also allows, "that the evidence in favour of contagiousness in plague may be originally traced to the East;" but as if afraid that his witnesses, the Moslems, should be deemed worthy of credence, he proceeds:

"The credulous character of the Orientals is well known. To say nothing of the gross religious superstitions of the Levantines, the

belief in peris, vampires, djins, and ghosts, is almost universal. I amused myself with collecting such superstitious stories as fell in my way, [we wish the Doctor had done nothing but amuse himself in this innocent manner,] and for any one fact which was adduced in proof of the contagiousness of plague, I found ten which proved the existence of peris; that they had been known to visit certain districts, intermarried with mortals, provided them with food, transported them through the air, and given abundant evidence of their presence and their power."

Comment on such argument is unnecessary, and we cannot allow the Doctor, after thus shewing the incompetency of his own witnesses, to produce them again; but we may mention one individual, whose mind is as free from any superstitious notion as is possible for that of a believer in the prophet of Mecca to be, Ibrahim Pasha, and he left no doubt on any one's mind that he firmly believed in the contagiousness of plague, when at a public audience, on Dr. Bulard being presented to him in company with three other medical gentlemen, it was mentioned that he had taken every means of exposing himself to plague, both by contact and inoculation, he replied, "well, what does all that prove but that he was the greatest fool of the four!"

All the proofs brought forward of the non-contagiousness of plague are and necessarily must be negative, whereas those on the opposite side are positive. Thousands may be exposed to the influence of contagion, and but one may be in circumstances capable of being affected by it, but that one exception proves the fact; whilst, on the contrary, all the examples quoted of persons attending closely on the sufferers escaping, really proves nothing, or does worse, by inducing mischievous doubts.

Dr. Bowring states that plague enters into the closest preserved quarantines, without any communication externally; in answer to which we would refer our readers to the paper by Dr. Bulard in our former Number, in which he as distinctly states, that all the places where actual isolation was observed escaped, and that it was only when, by some secret communication, the quarantine was broken, that the disease entered into such places, instances of which are given.

Faulty as are the Eastern lazarettos and quarantine establishments, still we think the picture given of them in this pamphlet is somewhat overdrawn.

"The fact is, the quarantine establishments are for the most part instruments, and terrible instruments, of diplomacy and state policy. Under the plea of a regard for the public health, all letters are opened, all travellers are arrested and imprisoned, all commodities are subject to regulations the most unintelligible, costly, and vexatious. I once was admitted to a lazaret in the Austrian frontier, where I

saw the correspondence of the East with England delivered to the authorities ; every letter was opened, examined, fumigated, re-sealed, and dispatched. In some lazarets the letters are only punched and smoked, in others they are cut across with a sharp instrument, and dipped into vinegar and water, so that the writing is rendered frequently illegible. Multitudes of letters sent by private hands, or other channels than the post, escape the quarantine altogether."

Now in answer to all this we would say, that we think it very unlikely that the English government would submit to having its letters opened by any other government passively, and it puzzles us to conceive how letters entrusted to private hand can escape the quarantine, when the bearer of them must of necessity submit to enduring it.

That quarantine lazarettos are improperly and carelessly conducted cannot be assumed as any argument against their utility if well conducted, but carelessness is not the accusation brought against them by Dr. Bowring, on the contrary, he appears to be annoyed at the great particularity with which the regulations are enforced. He proceeds then to inquire :

"Have lazarets, the best, the most rigid, succeeded in shutting out, or keeping down the plague? by no means. Odessa has frequently been quoted as having one of the best organized quarantine establishments in the world, certainly one of the severest. Yet, not long ago, the plague broke out in the lazaret, entered the town, destroyed a number of inhabitants, and ceased at a particular season, as it usually does."

Now we would ask, what is there wonderful in all this? that the disease should first appear in the lazaret, was the natural consequence of its being the first place where those who had come from an infected country were received, and that it was from thence communicated by some improper access with the town there can be little doubt, unless our author would have us to imagine that a ship was capable of conveying an uncontagious epidemic or malaria from one country to another. It strikes us forcibly too, that the epidemic nature of disease is assumed as a reason for considering it not contagious; this we cannot allow, and the fevers of our own country sufficiently prove the coexistence of those two properties. But the faulty government of lazarets is explained in a single phrase by our author, "that universal bribery and corruption exists all over the East." Such being the case, we may fairly say that, on the Doctor's own shewing, the medical men being ignorant, and the lazarets being ill-conducted, and accessible to bribery, the premises which he has taken are absurd, and nothing but an absurd conclusion can thence be deduced.

In Egypt and Syria, Mehemet Ali has made vast efforts to establish strict quarantine, but finding the great difficulty of enforcing their regulations on his own subjects, he wisely placed them under the control of the European Consuls; notwithstanding this regulation, strict quarantine is, I believe, seldom, if ever, observed.

It seems a very general opinion amongst those who consider plague to be only endemic, that the limits of its ravages are confined to Egypt, Syria, &c., on the southern and eastern coasts of the Mediterranean, but that it never is transmitted thence to Nubia, India, &c. Whether this be the case or not we cannot say, but a disease has been described in the first volume of the Transactions of the Medical and Physical Society of Bombay, which prevailed in Kattywar and in parts of the Zillah of Ahmedabad, in the years 1819-20, in the Eastern Indies, which bears all the characters of plague as observed in the Levant. These papers are peculiarly interesting, and from them we will make a few extracts, which in our opinion throw some light on this involved subject. Mr. Whyte, who made this communication to the Secretary of the Medical Board, states that this disease was known all over the district by the general name of "Ghant," at present raging (1836) in Moolee and Sila, identical with the disease of 1817, and three years ago prevailing for five months in the town of Morevee.

The same febrile symptoms, buboes, &c., which distinguish plague in Egypt, are to be remarked in this disease, with the same intractability and mortality. The inhabitants are stated not to consider the disease contagious, yet amongst the examples given is that of a Mussulman, who took care of cows, the members of whose family were consecutively attacked, first the father, two of the children two days after, and the rest, to the number of seven, successively at about the same interval; two perfectly escaped. Of these, three died and four recovered. Was this the effects of contagion or not? Mr. Whyte mentions that the Thakoor, or chief's brother, went about assisting, consoling, and handling the sick, without feeling any bad effects therefrom, thereby showing that casual intercourse is occasionally unattended with danger. If this were not the case we should entertain doubts of the disease being plague, some persons in every instance appearing incapable of being contaminated by the disease; as instances we may mention Napoleon in Egypt, the Lord Mayor in London, and the robbers who despoiled the dead at Marseilles, not to mention the greater number of physicians who attend on the disease.

From Morevee the plague extended to a village called

Moolee, distant two coss, on the same side of the river, whilst a village at the opposite side, not a quarter of a mile distant, named Moorba, perfectly escaped; the latter village, however, occupied a high and airy site, the former was buried in jungle. It also extended from Morevee to Wunkaneer, eighteen miles distant, here it only lasted fifteen days, and was almost solely confined to one class of people, the Boras, whose occupation is the manufacture of cotton cloths. The greater part of the cotton was said to be imported from the surrounding country, and part of it from Morevee. The number of deaths was estimated at sixty amongst the Boras; and not more than four or five of all the other inhabitants suffered. Every one attacked, died. The Boras became so much alarmed at this severe, and at the same time so partial visitation, in which every one of them seemed doomed to destruction, that they all left the town, and went to live upon the mountain in the neighbourhood, leaving only those who were sick in their houses with one attendant upon each; the whole of these very soon died. Whenever they heard of a death, the friends of the deceased came down, and performed the last offices as speedily as possible, returning again to their abode in the mountain. Some of those who attended these funerals were taken ill; many could not have suffered, however, in this manner, for at the end of fifteen days after their removal the disease had entirely disappeared. Here we find an ignorant people, under the influence of terror, acting in a similar manner to that in which the Pacha of Egypt does when plague breaks out amongst his troops, instantly marching them some miles into the desert, thus separating them from their suffering companions, and giving them active occupation in marching and pitching a new camp, and suddenly changing the air, a circumstance capable in itself of modifying many febrile disorders.

Of all the cases described as occurring at Wunkaneer, which terminated fatally, the only difference from the plague of the Levant, was, that there were neither petechiæ nor carbuncles to be seen, the buboes and other ordinary symptoms were present.

The natives "had not the slightest opinion that the disease was contagious; indeed they were determinately of opinion that it was not so. One man, the Thakoor's brother, confidently assured me that contagion was not the cause of the disease, affirming boldly that no man could be a better judge of the question, for that none had greater experience; he having lost in his family eight women and one boy: 'No, no,' said he 'Oorta jooda,' contagion is quite a different thing. Another woman died in this man's house while I was here."

This man must have been a reasoner after Dr. Bowring's own heart !

" Nothing," proceeds Mr. Whyte, " can show more clearly the evil arising from bigoted prepossession, than the case of this man's family ; for it is difficult to conceive what he would have considered as satisfactory proof of the reality of contagion. To the unprejudiced observer, the probability will appear very strong, that these women caught the disease one from the other, and that the boy got it likewise from them."

It is a curious coincidence, that a superstition equally favourable to the spread of plague, as the fatalism of the Turks, should exist here ; viz. an opinion that this was not a disease but an order from the Almighty to remove whomever it attacked, and that therefore all resistance was wicked or vain.

Mr. Whyte attributes the virulence of this disease in Kattywar to the greater number of the inhabitants living in close, ill-ventilated places, to their dirty habits, and to their constant association with their cattle ; which, as they are forbidden by their religion to kill them, increase to a great extent, grow old, and become affected with the most revolting diseases. In speaking of the agents supposed capable of producing it, Mr. Whyte observes :

" It may perhaps facilitate our inquiry into the cause of the present disease, distinctly to point out what at least have not been agents in its production. We may then at once and altogether exclude from consideration all putrefying animal and vegetable substances, all effluvia from jungles and marshes ; for every one of the towns that I have visited are in a remarkable degree exempted from every thing that could produce these, their situation being either on the rocky bank of a running stream, or on a dry sandy soil."

Having advanced the supposition that plague might arise from collections of putrid animal miasmata as in " Gaol," " Hospital," or " Ship" fever, Mr. Whyte says :

" I am more inclined to believe *that the disease was imported into this country*, and with this belief, that of the people in general coincides. There appear to me to be a great many facts which powerfully support this view, and which could not satisfactorily be accounted for in any other way. It is not known to have attacked any two places simultaneously, however much alike in those local causes, of which we have already admitted the probable influence ; after appearing in one town, it went after a little time to the town nearest, as from Limrea to Wudwan, Sila and Moolee this year ; or from Morevee to Wunkaneer in 1817, affecting slightly the intermediate villages."

“The class of people first and principally affected both at Sila and Wunkaneer, affords an additional presumption, that in these instances the disease was imported; for it is in every way probable, that the cotton was the vehicle by which the contagion was conveyed. The sudden disappearance of the disease from Wunkaneer, upon the separation of the healthy from the sick part of the people, cannot be satisfactorily accounted for, by any other supposition than that of imported contagion.”

“If local causes had produced the disease, why was their operation not evident again, after the return of the Boras to their houses, for in so short a space no improvement in their condition could have taken place, unless a diminution in their numbers be considered as one? Under such circumstances, again, the fact of the disease disappearing and reappearing after the lapse of years, cannot be accounted for. It is probable that during the period of the disease being absent, these local causes remained much the same; but on the other hand, if we believe in imported contagion, the difficulty is at once removed.”

The rules for the prevention of plague proposed by this gentleman are:

“Such as would secure cleanliness, and a complete separation of every family in which the disease had appeared, from the remaining sound population; they ought to be compelled instantly to depart from the town, and live in the open plain, under a temporary encampment. For the lower orders it would be well if the clothes of the diseased were to be burned. Experience has proved, that if not crowded in such a situation, the disease would not spread; and there can be no doubt, the beneficial consequences would soon be a subject of joy and congratulation to every one who had a share in promoting so benevolent a purpose.”

We find also remarks made on the same disease, in the same place, and in Kutch, by J. M'Adam; in which he takes nearly the same views of the subject as Mr. Whyte.

“The origin of this disease, which has for some time raged in Wagur, and the Mucha Kanta, is involved in obscurity. It appears to have been first noticed at Kuntakate or its neighbourhood, in May, 1815; thence it spread to Munsurr, Chilore, Andovee, and Wandia, all towns in Wagur; and in all of which it committed great ravages during the months of January, February, and March of the present year. In May last, after it had ceased to prevail in Wagur, it made its appearance in the town of Moorevee, on the Kattywar side of the Run; and there it has continued up to the present time to prevail with fatal effects. In August the disease repassed the Run to Buchan, twnty-three miles distant from this place, and lately appeared at the village of Chiroee, ten miles nearer. It also rages at

present in Rhadanpore, and has extended its ravages to the province of Sind, where it is said to be the cause at present of great mortality."

"One case of the disease occurred in this town; the person affected came from Buchan, and died the day after his arrival; but the precaution being immediately taken of having all those who had any communication with him sent out of the fort, and his house fumigated and untiled, so as to expose it to the sun, it did not again make its appearance; another person died outside the fort—all communication between it and the infected places having been prohibited."

This single case is in itself equal to a host of negative cases on the other side of the question, and proves incontestibly the value of strict quarantine and cleanliness.

Mr. M'Adam makes a remark, that those Hindoos engaged in the expression of oil are not liable to infection; this coincides curiously with a practice observed to prevail, by the author of this paper, amongst the native physicians of Egypt, that of anointing themselves with oil previous to touching a person affected with plague, as well as causing it to be used on their patients, mixed with various aromatics.

This gentleman also mentions, that precautionary means had been adopted by the Resident at Cutch, which hitherto had prevented the introduction of plague into that town. This was thought the more necessary, as Cutch is an intervening station between the large sea-port towns of Moondra and Mandavee which have an extensive commercial communication with almost every place on this side of India, and where, if plague once gained a footing, it would be impossible to say to what extent it might be propagated.

"Since writing the above account, information has been received that this *contagious disorder* is now raging with great mortality in Hydrabad, the capital of Sinde; a letter from that place mentions, that the daily number of casualties amount, to sixty or seventy. I have also just heard that the Right Hon. the Governor in Council has adopted precautionary measures to prevent the introduction of this disorder into Bombay.

Mr. Gilder, in speaking of the plague which raged in the Zillah of Ahmedabad, having mentioned its progressing from town to town, says:—

"It should be remembered, that whilst this work of destruction was going on, the rains were pouring down in torrents: the whole surface of the country between each village, which is generally situated on a rising ground or artificial hillock, presented to the view but one sheet of water. [How similar this to the Fellah villages in Egypt!] The communication thus impeded between different parts of the country, confined the inhabitants within the precincts of their

respective habitations, and compelled them, as it were, to inhale an atmosphere already overladen with morbid effluvia; additional strength was thus given to the disease, and its victims were, as may readily be supposed, proportionally numerous."

All the Indian writers unite in declaring, that this scourge is principally confined to the Brahmins, Banyans, Soonars, Durzees, and Koombhees, who subsist entirely on vegetables, those using animal food generally escaping. Mr. Gilder having described the various forms of this disease, thus speaks of one of them, and as it bears on the subject of contagion we insert it:—

"This form of the disease was introduced into the village of Bulle in the end of December last by a Banian of the place, who had returned from Dewpoora, whither he had been on business, and where it was raging at that time. The night after his return he complained of great uneasiness, pains in his loins and joints, attended with fever; these symptoms continued increasing throughout the night, and the next morning he perceived swellings of his inguinal glands, which were exceedingly painful; during the day he became delirious, and complained of intense thirst; no abatement occurred on the third day, on the evening of which he died."

Now mark the consequences.

"Every person belonging to this man's family, both male and female, died within the space of ten days of the same disease, which spread to the other two Banian families in the village, and committed similar ravages, in all destroying about thirty."

Now what can the non-contagionist say to a positive case of this kind? a man in good health visits an infected place, brings back with him a disease of which he dies, those most in contact with him next become ill and die, then those who follow the same occupation, and live in the same part of the village; and lastly, some of the general inhabitants.

Dr. Bowring, in his Essay, labours to prove that the plague is of spontaneous or endemic growth, and that, therefore, quarantines and lazarettos cannot prevent its progress; but like Doctor Bowring, we cannot make history a dead letter; we cannot forget the evidence of Sydenham, that previous to 1665 it usually invaded England at intervals of from thirty to forty years; that in that year it destroyed more than 8000 persons in London alone in the space of one week; and we would not risk its recurrence; that Marseilles suffered its twentieth visitation in 1720, and was fearfully devastated; that Moscow suffered similarly in 1771 and 2; Noja, in the Neapolitan dominions, in 1815 and 16; Venice in 1818; Silesia in 1819.

That the disease, be its nature what it may, is capable of being conveyed again to our shores ; and whilst we remember this, we also feel fully convinced, that a well conducted quarantine, and a proper airing and exposure of goods brought from infected places, is the only means which we possess of preventing the access of this dreadful malady.

It is natural to imagine that the length of the voyage should diminish in a certain degree the chances of vessels carrying contagion from land to land ; if this be so, the great advance which steam navigation has made, by virtually bringing us nearly to those countries where plague originates, or rather where it is continually perpetuated, increases in a very great degree the danger of contagion being carried once more to devastate our native land.

Doctor Bowring lays great stress on the supposed falsity of the arguments which have been adduced by many, that the plague generally appears in Egypt at the same time with the march of the Hadjes, and calls on the evidence of Mr. Abbott, one of the Pacha's Naval Surgeons, in a series of questions, amongst which he reports him as stating, that he considers plague indigenous in Egypt.

“ Indeed, I do not believe Egypt is ever entirely free from the plague, that is in its least virulent form.”

Here there is manifest inconsistency, and that the pilgrims may escape the accusation of introducing it, he says :

“ The idea of its being imported by the Hadjes must be incorrect, or we should have it occurring at all seasons of the year, instead of between November and June, in consequence of the arrival of the pilgrims being in the month of Ramadan, which does not occur always at the same season of the year.

Now if the disease always exists in a greater or less degree, it is much more likely that peculiar states of the atmosphere at the summer season, combined with the crowd of the pilgrims, may cause a very great increase of virulence in the disease ; however, the best authors, and the popular opinion, are both strongly in favour of a great increase of pestilence during this march.

There are many subjects more in this paper, which we would willingly touch upon, did space allow, and we would willingly concur with its author in the propriety of an inquiry being made by competent persons in the Levant and elsewhere, to determine much which is at present disputed with regard to this fearful disorder ; but those persons should be unprejudiced, and we fear much, that in the present age there are very few

medical men to be found, who, although they may fearlessly brave the disorder in their own persons, do not entertain secret misgivings on the subject of its contagiousness.

In conclusion, we will illustrate the argument most frequently made use of in this paper, to which we cannot yield our concurrence. The author of this critique, in making a cursory visit to Egypt three years ago, made some dissections of subjects which died of plague; on one occasion, in the month of June, his servant, a Greek, was engaged for about half an hour in holding up some intestines whilst he made sketches of them. In the course of the afternoon this man became ill, exhibited all the symptoms of virulent plague, and died in three days. Now Dr. Bowring would draw as conclusions from this case, that the disease was not contagious, because *I* had *not* died too, whilst the contagionist would rest satisfied by tracing the positive occurrence to its probable cause, and regard the impunity which *I* suffered as the accident. This little episode may be applied to almost all the reasoning which has been employed on either side of the controversy. However, in our obstinacy, prejudice, or by whatever name it may be called, we confess we prefer one solid fact, which warns us of danger and risk to be run, to a thousand speculative opinions, however brilliant, and the more particularly when we consider, that these opinions are fraught with hazard, and, if listened to, are likely to produce not only confusion, but inevitable distress.

Transactions of the Medical and Physical Society of Bombay.
Bombay, printed at the American Mission Press, 1838.
(J. M. Richardson, 23, Cornhill, London.) Vol. I.

WE have much pleasure in acknowledging the receipt of this interesting volume, the first of, we hope, a long series. Its contents are very instructive, and do much credit to the Society; we regret that we cannot do more than refer the reader to the following recital of the papers it contains, as taken from the chapter of contents. We have elsewhere used some of the materials supplied by the third, fourth, and fifth articles, to refute the dangerous speculations of Doctor Bowring, who has lately advocated the rash measure of abolishing quarantine in England.

“CONTENTS. I.—A General Sketch of the Province of Guzerat from Deesa to Damaun. By A. Gibson, Esquire, Vaccinator of the Deccan Division.

“II.—Observations on the Climate of the Mahabuleshwur Hills. By J. Murray, Esquire, Surgeon of the Convalescent Station.

“III.—A Report on a Disease which prevailed in Kattywar and in parts of the Zillah of Ahmedabad, in the years 1819 and 1820, addressed to the Secretary of the Medical Board. By Thomas Whyte, Esquire, formerly of the Bombay Service.

“IV.—A Report on a Disease which prevailed in part of Kutch and Kattywar in the years 1815 and 1816. Addressed to the Secretary of the Medical Board. By J. M'Adam, Esquire, Superintending Surgeon Deccan Division, formerly Civil Surgeon in Kattywar.

“V.—A Report on a Disease which prevailed in part of the Zillah of Ahmedabad in the years 1817, 1818, and 1819. Addressed to the Secretary of the Medical Board. By J. Gilder, Esquire, formerly of the Bombay Service.

“VI.—A few Remarks on the Diseases in the Hospital of the 3rd Troop of the Horse Artillery at Deesa, during the months of August, September, October, November, and December, 1834. By R. Brown, M.D., Assistant Surgeon, European General Hospital.

“VII.—Observations on Dracunculus, extracted from the half yearly reports of the Diseases prevailing at Dharwar, in the 1st Grenadier Regiment, in the year 1836. By D. Forbes, Esq., Surgeon.

“VIII.—Historical Report of His Majesty's 2nd or Queen's Royal Regiment, for the year 1836. By R. H. Hunter, Esq., Assistant Surgeon, H.M.'s 2nd Regiment.

“IX.—Cases of Cardiac Disease, and of Tubercular Phthisis, occurring in His Majesty's 2nd, or Queen's Royal Regiment. By R. H. Hunter, Esq. Assistant Surgeon, H.M.'s 2nd Regiment.

“X.—A few Notes on the Thermal Springs in the Konkan. By A. Duncan, Esq., Surgeon.

“XI.—Practical Surgical Cases. By W. B. Barrington, Esq., Civil Surgeon, Broach.

“XII.—Notes on the State of Medical and Surgical Science in the Countries bordering on the Indus. By Percival Lord, M. B., in Medical charge of the Kabul Mission.

“XIII.—A Selection of Cases of violent Death which have formed subjects of investigation before the Coroner of Bombay. By J. F. Heddle, Esq., Surgeon to the Coroner.

“XIV.—A Letter on the Manner of breeding Leeches practised in the Deccan. Addressed to the Secretary of the Society. By A. Gibon, Esq. Vaccinator, Deccan Division.”

The India Journal of Medical and Physical Science. Edited by FREDERICK CORBYN, Esq. New Series. Vol. II. Calcutta, 1837.

THREE medical journals now appear in India, of which two are published at Calcutta, and one at Bombay. Doctor Corbyn's Journal has been long established, and is much quoted in America and Europe. Besides original articles, it contains analytical reviews of the most valuable publications which emanate from the press in Europe, and thus puts its readers in possession of the present state of science. It is admirably conducted. We have not yet seen a copy of the other Journal published in Calcutta, and edited by Doctor O'Shaughnessy, a gentleman of well known ability. Altogether it is most cheering to witness the efforts now made to cultivate the vast field open to the observation of our countrymen and colleagues in the East.

SCIENTIFIC INTELLIGENCE.

Institution of the Pathological Society of Dublin.—It gives us the greatest pleasure to announce, that this Society has been organized, and is now in full operation. We entertain no doubt that this Society, constituted as it is, will exercise the best influence on the School of Dublin. Its objects are two-fold; first, the exhibition and registering of the various specimens of morbid anatomy, which are met with by the members in their private and hospital practice; and next, the extension of the advantages of this exhibition to students without distinction. Three meetings have already been held, which have excited the greatest interest. We shall give reports of the meetings of this most important Society in our next Number, and in the mean time subjoin the list of officers and fundamental regulations.

“I. *Name and object of the Society.*—This Society shall be denominated the Pathological Society of Dublin, and shall have for its object the cultivation of Pathological Anatomy, particularly with reference to the diagnosis and treatment of disease.

“II. The Society shall meet on every Saturday, at four o'clock, at which meetings, recent and prepared specimens of anatomy, casts, and drawings may be presented, accompanied with a short description. Any additional information which the Secretaries may require, to be given to them in writing.

“III. The Society shall have no claim upon any specimen presented, but a registry shall be kept of its nature, and of the name of the person who presented it.

“IV. A Report of the Proceedings of the Society shall be prepared by the Secretaries, and published every two months.

“V. Each member of the Society shall be permitted to admit three visitors by tickets, so as to give Students the advantage of being present at the meetings of the Society; the tickets to be signed by the Secretaries, and delivered at the door.

“*Presidents* :—Mr. Carmichael; Mr. Colles; Mr. Crampton; Dr. Cusack; Dr. Graves; Dr. Marsh.

“*Vice-Presidents* :—Mr. Adams; Dr. Montgomery.

“*Secretaries* :—Dr. Stokes; Mr. Smith.

“*Treasurer* :—Dr. Hutton.

“*Members of Council* :—Dr. Beatty; Dr. Carlile; Dr. Corrigan; Mr. Ferrall; Dr. Green; Dr. Harrison; Dr. Hutton; Dr. E. Kennedy; Dr. Law; Dr. Macartney; Mr. Smith; Dr. Stokes.”



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